THURSDAY, JULY 28, 1870

NATURAL HISTORY IN SCHOOLS

AMONG the many indications of the taste for natural science which is spreading throughout the country, there is none more striking than the rapid increase, during the last ten years, of local Field Clubs and Natural History Societies. Those who reside in our larger towns and can avail themselves of every facility for prosecuting their favourite branch of study, are apt to under-estimate the value of these bodies, and to look upon them as merely accessories to the bringing together, under the name of science, of men of congenial tastes, rather than as performing any definite or actual work; but this is an error which can only arise in the minds of those who have had no opportunity of seeing the working of such societies, and who have not therefore felt the advantages by which membership is attended in a remote country district. No doubt there are, here and there, Field Clubs which fall short of the perfection they ought to attain; but that these are the exception, and not the rule, no one who has investigated the matter can reasonably doubt.

It is not, however, to such societies that we would now direct attention, so much as to the rise and progress in our public and private schools, of bodies whose aim and object is the same; a progress which is the more gratifying when we remember that it mainly originates with the boys themselves. As to the value of such societies there cannot be two opinions. We would not for a moment depreciate cricket and other manly sports; but we would supply for the mind a rational substitute for such manias as that of postage-stamp collecting, which a short time ago reigned in our schools.

It may not be known to many who are aware of the existence of such societies in our larger schools, that their origin dates back nearly forty years. It is to the York School, under the superintendence of the Society of Friends, that we must look for the first step in this direction. In 1834—three years after the formation of the Berwickshire Field Club, then the only one of its kind in the kingdom—a Natural History Society was formed in the York School, which is in operation at the present time. Quiet and unpretending, few even of those who take an interest in such societies are aware of its existence, or have seen the modest report which it issues year by year. Had Mr. Carlyle, in his schoolboy days, shared such advantages, we should not have him now lamenting that no one ever taught him the constellations, or "made him at home in the starry heavens!" Looking through the names of members, formerly pupils at the school, we note among them men now known to science; and we shall scarcely be wrong in supposing that science is in a great measure indebted to the York School for the work which they have done, and are still doing.

Turning now to our public schools, the first which claims our notice is Marlborough College. In 1863, the Rev. T. A. Preston, one of the masters, assisted by some of the pupils, published a Flora of Marlborough, "with a hope that, by placing before the members of the college a proof of the botanical riches of our neighbourhood," he might induce some of them to take an interest in the study of botany, and perhaps indirectly through this, of some other branch of Natural History. How far this hope was realised may be gathered from the fact that, in a few months "a spontaneous movement arose among certain members of the school towards the pursuit of Natural History with some degree of system and organisation." The *Marlborough College Natural History Society* was speedily formed, Mr. Preston being elected president, and to him we are indebted for a complete set of the reports issued by the Society, containing the rules, the papers read, and the calendars—ornithological, botanical, and entomological, for the various years. As it is mainly upon the model set by this Society that other similar ones have been formed, a few details may be interesting. The rules strike us as being particularly good, and might be adopted with advantage by Field Clubs generally, although some of the best of them, such as that providing for the exclusion of members who do not evince sufficient energy in the working of the club, could only be satisfactorily carried out in school societies. Glancing through the papers, their general excellence, as well as the variety of subjects, is worthy of note. Not that all are of equal value; this indeed it would not be reasonable to expect; but taking them as a whole, they would be creditable to members of "grown-up" societies. Due prominence is given to those bearing upon matters of local interest, and new discoveries in science are brought before the members. The various calendars are most carefully compiled, and are so printed as to indicate each year's additions to the flora and fauna of the district; the number of members cited as contributing gives us a good idea of the actual work done by the Society. Besides these transactions, a museum has been established, and judging from the numerous contributions acknowledged in the reports, it is assuming very satisfactory proportions. The formation of a museum is a work peculiarly suitable to a school; a common centre, so to speak, is provided, upon which to work; the objects collected are sure to be carefully preserved, and the collection is in a position where it may be of real service to those interested in it. A library has also been formed, and we may suggest to any of our readers who may happen to have on their shelves duplicate copies of any scientific work, that they cannot do better than forward them for the use of such a body.

The *Harrow School Scientific Society* was founded in 1865, but here, as at Marlborough, considerable interest had been manifested in Natural Science for some time previously, as was evidenced by the publication, in 1864, of a Harrow Flora, with chapters on the birds and insects, all by Harrovians. The printed reports give selections from the papers read, which, if not equal to those of the Marlborough Society, are still creditable; we have, however, no calendars, and the additions to the fauna and flora are comparatively few.

The *Rugby School Natural History Society* dates from March 1867, and has issued two reports. The most noticeable feature in the published papers is the prominence given to subjects bearing upon the Darwinian theory of the origin of species, and some extremely thoughtful essays on this, as well as on the more recent facts which support the theory, are printed. Some papers on the "Protective Resemblances," noticeable among British, as well as foreign insects, seem to us particularly

suggestive; the natural history of the district receives a due share of attention, and in the two reports before us we have lists of the birds, Lepidoptera, and flowering plants, with dates of appearance, record of new species, &c., besides a copious list of local Lias fossils. A museum and library are in course of formation.

The *Wellington College Natural History Society* held its first meeting on May 13, 1868, and its rules are based upon those of the Rugby Society. One report only has as yet been published, in which we find selections from the papers, including one by Professor Kingsley. Botanical and ornithological lists are given, but the comparatively recent formation of the Society renders any detailed criticism unnecessary.

Last comes the *Winchester College Natural History Society*, established in March last, a notice of which has already appeared in NATURE. Of course little has been done, and nothing published at present; but we learn from the secretary that the plan of working by sections has been adopted, and that the meetings are well attended. To this and to all similar bodies we heartily wish success.

We are sorry to see that Eton is "conspicuous by its absence" from our list. The publication, two years since, of "The Birds of Berkshire and Buckinghamshire," by Mr. Clark-Kennedy, "an Eton boy," induced us to hope that a school which in position is second to none, would not be left behind in the onward march; but as far as we can ascertain, nothing in the way of a society has been established at present. We shall be glad to learn that we are mistaken in this matter, and trust that, even should no such body at present exist, it may not be long before we hear of its formation.

THE RELATIVE VALUE OF CLASSICAL AND SCIENTIFIC TRAINING

Wodurch die humanistischen Gymnasien für die Universität vorbereiten? Rede an die Studirenden der Ludwig-Maximilian's Universität zu München gehalten am 4 Dezember, 1869, von Dr. Med. Max v. Pettenkofer, Professor der Hygiène, z. z. Rector. (München, 1869.)*

THE German-reading public can possess itself at a very trifling cost of a very weighty opinion as to the relative value of classical and of scientific training, by the purchase of an address delivered last December in Munich by Professor Max v. Pettenkofer, in his capacity of Rector or Chancellor of the University for the time being. There is in existence an English document (we fear we cannot speak of it as a *publication*) in the shape of a report, laid before the authorities of Owens College, Manchester, which has appended to it a name nearly, or quite, as familiar to the student and readers of NATURE as Pettenkofer's—viz., that of Professor Roscoe, and in which the same process of "ponderation" is applied to the classical "Gymnasia" and the modern "Real-Gymnasien" severally. V. Pettenkofer, who is not referred to in that report, shall here speak for himself, and we may say at once, that after stating more or less fully the objections which are ordinarily urged against the classical system, he declares himself an adherent of the party which stands *super antiquas vias*. The two delegates of Owens College appear to incline in the same direction somewhat, but are more eclectic and more careful in balancing their utterances as to the possibility of combining the two systems than either v. Pettenkofer, whom we shall forthwith cite on the one, or than Helmholtz, whom they cite on the other side.

The argument from authority has a legitimate place in questions concerning such matters as the genesis of culture and as the existence of capacity and capabilities; for in such questions neither the facts themselves nor the mode of their origination can be always looked upon as beyond the region of probability. But as we are writing in a scientific periodical, we will begin at least with something which admits of being quantitatively estimated; and we will do this by giving the time-tables of the classical (*Humanistischen*) and of the modern (*Real-Gymnasien*) schools in Bavaria, as we find them in v. Pettenkofer's address (pp. 5 and 18).

In classical schools, out of 99 hours per week:—

8	hours per week	are given to	German.
26	,,	,,	Latin.
22	,,	,,	Greek.
8	,,	,,	French.

(*i.e.*, 64 hours, or 65 per cent., are given to languages, three-fourths being Latin and Greek, and one-fourth German and French.)

17	hours per week	are given to	Mathematics.
10	,,	,,	History.
8	,,	,,	Religious Instruction.

In "Real-Gymnasien," out of 112 hours per week:—

9	hours per week	are given to	German.
14	,,	,,	Latin.
13	,,	,,	French.
4	,,	,,	English.

(*i.e.*, 40 hours, or 33 per cent., are given to languages, of which time only one-third is given to one ancient language, one-third to French, the other two-thirds to German)

27 hours per week are given to Mathematics.

(*i.e.*, algebra, elementary geometry, trigonometry, descriptive and analytical geometry and higher analysis, taking 22 per cent. of the whole number)

4	hours per week	are given to	History.
19	,,	,,	Natural Science and Geography.
24	,,	,,	Drawing and Modelling.
8	,,	,,	Religious Instruction.

The "Real-Gymnasien" are thus seen to exact 25 per cent. more hours than the classical schools; and it is by this increase on the one hand, coupled with a curtailment of the quota assigned to languages on the other, that time is found for mathematics and for natural science, with the drawing and modelling so indispensable to it. V. Pettenkofer deprecates the making of any material increase in the number of hours to be spent in the gymnasien, on the undeniable ground that the day is no longer, and man no stronger now than were the days and the men of 2,000 years ago; and space for such additamenta as must be made to the curriculum must be found by

* "In what way do classical schools give students a preparation for the University?" An address delivered to the students of the Ludwig-Maximilian University in Munich, on the 4th of December, 1869, by Max. v. Pettenkofer, M.D., Professor of Hygiène, and at the time Rector of the University. (Munich, 1869.)

bettering the methods and means for communicating instruction, and effecting thus an economy of time.*

The Bavarian chemist and hygienist does not himself suggest any ways and means whereby this economy may be effected, and presumptuous though it be, we will attempt to supplement this deficiency by saying that such an economy might be effected in England and English schools by applying one or other or all three of the following lines of treatment to the classical curriculum, even without cutting its Greek adrift. Latin and Greek, to put the boldest suggestion first, might be studied in certain, and those not a few, cases, as literatures and not as philologies; or, as a second alternative, when some training in philology is to be retained at whatever cost, such training might be made more intelligible and so less distasteful and wasteful of time, by making the study of it comparative, as recommended by Professor Max Müller in his evidence before the Commission just referred to; or thirdly, synthetical scholarship, in the way of verse-making, should be considered as a luxury and refinement to be reserved for the delectation and cultivation of those few who, in any age, show any aptitude for it, and synthetical scholarship in the way even of writing Latin prose might, due precautions having been taken, be dispensed with in the cases of youths who, whilst wholly incapable in that, had shown some capacity in some other line. Our "due precautions" should consist in the multiplying the practice of synthetical scholarship in the way of translation from Latin into English. We know the horror which these suggestions will excite in the breasts of schoolmasters of the type represented by the gentleman who told the Commissioners already referred to, that if he were set to teach History in set lessons, he "should not know how to do it." But we believe that by the adoption of any one of the three lines of action just glanced at, space and time might be found for the introduction of the natural sciences into the curriculum of any public school, and that at once without injury to the dignity of either the one or the other of the two sets of studies, and without injury to the physical or mental health of the learners.

But it is time, perhaps, that we should let v. Pettenkofer speak for himself; and this he does (at p. 12, *l. c.*) to the following effect:—"I am convinced that philology and mathematics furnish precisely the material for teaching and intellectual discipline which is essential for our gymnasia, and I look upon the material furnished by other sciences as mere accessories. I know that in putting forward this view, which I do not do now for the first time, I put myself into opposition with the tide of opinion which is prevalent just at present, and which anticipates great advantages from the introduction of additional subjects of instruction, and especially from the introduction of instruction in natural science into 'Latin schools (Lateinschulen) and gymnasia.'" Further on (p. 16) he proceeds as follows:—"The results of actual experience appear to me to favour my views. In other parts of Germany, experiments have now, for a long while, been made with gymnasia and similar institutions, in which much natural science is taught. But I cannot as yet discover that any remarkable number of persons who have subsequently distinguished themselves in natural science have come from these schools. In this matter reliable statistics of the pupils leaving (*der Abiturienten*) a Berlin gymnasium, the so-called 'Old Cologne Gymnasium,' in which natural science has for a long while formed part of the curriculum, would be very instructive. Distinguished men come, from time to time, from this gymnasium, but certainly not in greater numbers than from any other classical (*Humanistischen*) gymnasium where no natural science at all is taught. It would long ago have been a notorious fact if a disproportionate number of the younger professors of natural science in the Prussian Universities could have been shown to have been formerly students in the Cologne Gymnasium."

We imagine that this "Old Cologne Gymnasium," thus referred to by v. Pettenkofer, is none other than the "mixed" (*simultan*) school described by Mr. Matthew Arnold under the name of the *Friedrich Wilhelm's Gymnasium* at Cologne, in his "Schools and Universities of the Continent," pp. 218-221; and but that more antagonism and less familiarity subsisted between North and South Germany six months ago than, we are happy to think, subsists now, we apprehend that more would have been made of the history of this institution by the Munich Professor. For Dr. Jaeger, the director of this mixed school, who, as he had been refused a nomination to another school, the Bielefeld Gymnasium, by the Education Minister, on account of his politics, cannot be suspected of reactionary leanings, spoke to Mr. Arnold in the following sense (see p. 221, *l. c.*): "It was the universal conviction with those competent to form an opinion, that the *Realschulen* were not at present successful institutions. He declared that the boys in the corresponding forms of the classical school beat the Realschule boys in matters which both do alike, such as history, geography, the mother tongue, and even French, though to French the Realschule boys devote far more time than their comrades of the classical school. The reason for this, Dr. Jaeger affirms, is that the classical training strengthens a boy's mind so much more. This is what, as I have already said, the chief school authorities everywhere in France and Germany testify. In Switzerland you do no hear the same story."

With regard to Switzerland, we learn from the Owens College Report above mentioned that Professor Zeuner of the Polytechnic School at Zurich, holds that the establishment of "Real Gymnasia," or High Schools of Science, to take equal rank with the old classical gymnasia, and to put pure and applied science on the same footing for educational purposes as that which the classics enjoy in these schools, is a desirable thing, but that he allows that by the introduction of a "bifurcation" system into the older schools they might be made equal to meeting all modern requirements. Helmholtz, on the other hand, may in the

* It may be well here to lay an English programme of school-work alongside of the two above given schedules of German school-work. A scheme to the following effect may be found as suggested by the Public School Commissioners, in their report, vol. i., 1864, pp. 34, 35:—

In an English public school of 39 hours per week—

11	hours	are to be assigned to	Classics, History, and Divinity (lessons)
10	"	"	" " (preparation).
3	"	"	Arithmetic and Mathematics, *i.e.*, not less.
2	"	"	French and German (lessons).
	"	"	" " (preparation).
	"	"	natural science (lessons).
	"	"	" " (preparation).
	"	"	music and drawing.
	"	"	composition.

same report be found pleading strongly for "the foundation on equal terms of complete academic institutions for science" as a "counteraction of the tendency of classical men to lean on authority alone."

"Philological culture," says the eminent physiologist of Heidelberg, "has an ill effect on those who are to devote themselves to science; the philologist is too much dependent on authority and books, he cannot observe for himself, or rely upon his own conclusions, and having only been accustomed to consider the laws of grammar, all of which have their exceptions, he cannot understand the invariable character of physical laws." Granting with all respect the premises laid down by Professor Helmholtz, we should demur to the conclusion which he would base upon them, and profess ourselves unable to see that, because particular institutions had a tendency to dwarf and stunt particular faculties, they should therefore be left undisturbed to do this evil work uncounteracted. And still leaving the premises unimpugned, we should set up a cross-indictment to the effect that if classical studies left the student of them unacquainted with the invariability of natural laws, physical studies leave the student unacquainted with the variability of men's minds. But, so far as the business of life consists in having to do business and hold intercourse with our fellow-men, this acquaintance with the variability of men's minds is simply the particular kind of knowledge which is not only the most practically useful and marketable of all kinds of knowledge, but is precisely the kind which, by common consent, is allowed to characterise if not to constitute "culture."

Lord Lyttelton, however, and the Endowed Schools Commissioners would appear to be in favour of the establishment of locally distinct schools for the two sets of studies and of students, and herein to be at one with Helmholtz. The Owens College Delegates, on the other hand, are, like ourselves, in favour of a system of bifurcation, which would not necessarily keep apart persons of different mental conformation who might be much benefited by mutual contact. They have come to this conclusion mainly for reasons based on observations and testimony given in Germany. Our peculiar social organisation makes the question more complex for us; but we, too, have our experience as well as the Germans; and time has shown that an Englishman, whose reputation as an educationalist is equal to that of Helmholtz as a physicist, may, in this very matter, be as far wrong as we believe that great physicist to be. In 1864 Dr. Temple told the Public Commissioners (see Report, vol. ii., p. 312) that he should "not consider it wise to follow the Cheltenham and Marlborough examples by attaching to the public schools modern departments. The classical work would lose, the other work would not gain!" In 1867 we find a distinguished Rugby master, the Rev. J. M. Wilson, speaking to the following effect of the results produced by the changes set on foot in accordance with the proposals of the Public Schools Commissioners, and earnestly and honestly carried out. "Lastly, what are the general results of the introduction of scientific teaching in the opinion of the body of the masters? In brief it is this: that the school, as a whole, is better for it, and that the scholarship is not worse. . . . This is the testimony of classical masters, by no means specially favourable to science, who are in a position which enables them to judge. . . . It is believed that no master in Rugby School would wish to give up natural science and recur to the old curriculum." G. ROLLESTON

PAMPHLETS ON METEOROLOGY AND MAGNETISM

Journal of the Scottish Meteorological Society. (Blackwood and Sons.)

The Normal Winds of Bombay. By C. Chambers, F.R.S

WE have received the *Journals* of the Scottish Meteorological Society from the beginning of 1867 to the end of last year, and we find them, on inspection, to be full of a variety of interesting and valuable matter.

The Scottish Society does not confine its attention to one particular branch of meteorology, but is broad in its sympathies as well as energetic in the development of its objects, and it is no doubt owing to this that so much is done with comparatively small means, and so much ground occupied with advantage. Among the numerous papers which constitute these journals we observe an address by that veteran agriculturist, the Marquis of Tweeddale, "On the effects of solar radiation in relation to crops." Anything on this subject is interesting from one who has himself grown wheat on the fields of India, and baked it into loaves which were duly distributed to his various sceptical friends.

We note with pleasure a proposal by the noble author for two experiments on the physiological branch of meteorology, firstly, What portion of the value of the sun's direct rays is due to heat, and what to light? and secondly, Whether the heat is of value as applied to the roots in the soil, or as regards its stimulating effects on the plant above ground?

The indefatigable secretary of the society, Mr. Alex. Buchan, contributes many interesting papers, and among them a series on the well-known interruptions in the regular rise and fall of temperature in the course of the year. Six cold and three warm periods are discussed and the author arrives at the following conclusion:—

"The unusually cold or warm periods which occur with considerable regularity at certain times of the year have, so far as we have examined them, been proved to depend on the relations of the polar and equatorial currents to each other. And the circumstance that one of these great atmospheric currents and not the other prevails over this portion of the earth's surface at stated seasons, is a valuable fact in meteorology, particularly in the light it seems to cast on the periodicity of weather changes."

In another memoir, Mr. Buchan discusses the cold weather of March 1867, which he attributes to the unprecedentedly high atmospheric pressure which prevailed in the north and north-west of Europe from the beginning to the 24th of the month.

Mr. Thomas Stevenson, in another very valuable and original paper, introduces the method of Barometric gradients as a means of ascertaining the intensity of storms. Very probably it may ultimately be found that we can measure a storm better by the Barometric differences which cause it than by the violence of the wind which constitutes it a storm, but the first step is surely to measure directly and accurately the intensity of storms considered as independent phenomena, and the second

to trace the connection which subsists between storms and Barometric differences.

We have also in these journals papers on Ozone, from Dr. Mitchell and Prof. Crum Brown; and in the physiological branch of the science we have observations on crops, trees, birds, &c., besides an interesting paper by Dr. Mitchell, on the cause of some of the pernicious effects of polar winds.

Besides the *Journals* of the Scottish Meteorological Society, we have received two pamphlets written by Mr. Charles Chambers, Director of the Bombay Observatory, which are of much value to men of science as thoroughly scientific discussions of phenomena observed with instruments of precision. In one of these, entitled "The Normal Winds of Bombay," we have a full analysis of the climate of that part of India as far as the element of wind is concerned.

In another pamphlet by the same author, forming part of the *Transactions* of the Royal Society of London, we have the magnetic phenomena of Bombay discussed in a very able manner after the method first introduced by General Sir E. Sabine and followed now by most magneticians. If we have yet made little advance in assigning the causes of magnetic variations, we have at least in such pages as these a solid foundation upon which to build, and our progress in terrestrial magnetism in this respect contrasts favourably with what we have achieved in former years.

BALFOUR STEWART

DONKIN'S ACOUSTICS

Acoustics: Theoretical. Part I. By W. F. Donkin, M.A., F.R.S., &c., Savilian Professor of Astronomy, Oxford. Pp. 202. (Clarendon Press, 1870.)

IF the Delegates of the Clarendon Press are able to carry out their programme, it will be possible before long for English students to learn Physics in their native language. Besides Thomson and Tait's "Natural Philosophy," which, if completed in the same way as it has been begun, will be a book for a nation to be proud of, they promise us a series of separate educational treatises on the several branches of Physics. Of these there are already published Dr. Balfour Stewart's excellent treatise on Heat, and the work mentioned at the head of this notice. This, unhappily, is only a fragment, forming part of the general theoretical introduction to a treatise on Sound and the principles of Music which the author did not live to write. But, although its quality is such as to make us keenly sensible of the loss which English science has suffered by the author's removal before he had completed the work, the part that is published treats of subjects of so fundamental a nature, and so little dependent on what would have followed them, that its intrinsic value is probably not much lessened by the absence of the remainder.

The first chapter begins with a general description of the mechanism of the ear; this is followed by an explanation of the mode of representing vibratory movements by periodic curves, and a discussion of the nature of pitch, and of the principle of the superposition of vibrations, as bearing upon the distinction between noises and musical sounds, and upon the general mode of perception of sounds by the ear. The second chapter is headed "Miscellaneous Definitions and Propositions," and is chiefly occupied with the mode of defining musical intervals, and with the statement of their most important relations. The third chapter treats of the analytical representation of simple harmonic vibrations, and of the composition of vibrations at right angles to each other. The fourth chapter treats of the properties of the *harmonic curve*, of the composition of harmonic curves, and of Fourier's Theorem. The fifth, sixth, and seventh chapters are devoted to the vibrations of elastic strings, the greater part of the sixth being occupied with the description and experimental treatment of the important subject of *forced oscillations*, and an appendix to the same chapter with the mathematical theory of them. The eighth chapter treats of the longitudinal vibrations; and the ninth and last, of the transverse vibrations of elastic rods. The greater part of the book is addressed to mathematical readers, but much of it may be read with profit by students whose mathematical acquirements are very moderate. The evidence throughout the work of the author's mastery of his subject gives to it a freshness and individuality which are in strong contrast to the characteristics of the undigested compilations which form so large a part of the literature of physics.

C. FOSTER

OUR BOOK-SHELF

Daily Readings in Natural Science. By Rev. J. Robertson. (London: C. Bean. 1870.)

THIS book has been prepared for beginners in the study of Natural Science; it is clearly and pleasantly written; each day in the week during a term has its subject allotted to it, either on Natural History, Physics, Botany, Astronomy, Natural Phenomena, Chemistry, Geology, Manufactures, Animal Physiology, or Applied Chemistry. The chapters are short, with questions at the end of each for the use of teachers. Mr. Robertson has written several other similar books, which he uses in his own school, and his success as a teacher lends great weight to the following extract from the preface:—"It is hardly necessary to descant lengthily upon the advantage of introducing science teaching into schools. The author, however, may be pardoned for giving his experience of adding science to the usual course of studies among his own boys. The science classes in his school had not long been established before he found that those boys who took no interest in their ordinary work soon manifested a quickness and brightness in dealing with natural objects that was quite remarkable, so much so, that after the first three months he doubled the time devoted to science by the upper form, and commenced new classes for the benefit of the middle and lower forms. Speaking generally, the study of Natural Science quickens a boy's powers of observation and comparison; he learns to express his thoughts in proper logical order, his judgment is developed, and the tendency that all boys have to form hasty conclusions is checked and tempered." To these we may join the following practical hints on conveying scientific instruction to schoolboys, in a notice to teachers at the end of the book:—"To be of real use science must be taught practically. Experiment and deduction should go hand in hand, and a boy ought never to be called upon to commit a fact to memory the truth of which he has not previously seen demonstrated. This of course presupposes perfection in the way of apparatus and specimens, a state of things that may possibly exist some day at such magnificent centres of learning as Eton, Harrow, or Rugby, but the boy who by circumstances is obliged to pass his days at smaller establishments, must take for granted a large number of the facts with which he stores his memory. But the ingenious teacher will see a thousand ways of demonstrating facts to his pupils with the outlay of very little money or time. The short course of

Natural History given here should be supplemented by visits to the Zoological Gardens and the British Museum. Having gone through the first three lessons, the cleverest boy in the class, or the teacher himself, should note down the names of the different animals described, with their peculiarities; the notes might then be copied by each boy, or at any rate read out to them. The visit to the Gardens or Museum should next be paid, not with the idea of wandering about in a desultory manner, but with the object of testing the truth of the statements contained in the lessons. Half an hour with a rabbit's or sheep's head, the examination of the teeth of the cat or dog, will help wonderfully to develop a boy's love for Natural History. The lessons should if possible be illustrated by the skulls or skeletons of some of the smaller members of each order or class. Such specimens may be obtained at a cheap rate from Mr. Cutter, 35, Great Russell Street, Bloomsbury. The same principle may be followed with Astronomy." These extracts show that the book is the work of a practical man, and as such we commend it.

LETTERS TO THE EDITOR

The Editor does not hold himself responsible for opinions expressed by his Correspondents. No notice is taken of anonymous communications.]

Spontaneous Generation

PROF. WANKLYN says the opponents of spontaneous generation are placed in a dilemma by some new facts and arguments recently advanced in favour of the theory by Dr. C. Bastian, whose observations were only concluded in NATURE for July 14, 1870.

Prof. Wanklyn might have allowed the "opponents" a little more breathing time, and if his cause is really so strong, why so soon call attention to "other difficulties" under which they labour? These tactics compel me to remark that the spontaneous generation theory will gain nothing by being forced upon the attention. Like other doctrines it must stand or fall according as the facts on which it is based are confirmed or refuted. It matters not whether A or B is an advocate for or against. The question can be determined by observation and experiment only. It is of little importance though the theory be supported by the press or be believed in by a large section of the public. If it can be proved to be true it will be accepted, but the utmost notoriety it may be possible to gain for it cannot create conviction of its truth.

I confess to being an "opponent" of the doctrine but simply because I cannot admit that the evidence yet adduced is at all convincing; while it seems to me the very way in which the doctrine has been thrust forward is calculated to excite undeserved distrust. The attempts made to prepare our minds, as it were, for its reception, the frequent announcements that the proof is *coming*, the bolstering up, if I may use such a term, this doctrine appears to require—instead of promoting its reception, is only calculated to excite suspicion, which may be entirely undeserved. I, for one, am quite prepared to receive the facts adduced in favour of the doctrine, but am by no means disposed to accept immediately or unconditionally the inferences drawn therefrom.

Before I proceed to discuss the "dilemma" in which, according to Prof. Wanklyn, as an opponent of the doctrine, I am placed, I would suggest that figures of the different forms of organisms, supposed to be *spontaneously generated*, be published in NATURE, side by side, with a short description, reference to authority, and magnifying power. In this way we should see at a glance the different kinds of organisms which had been formed *de novo*, according to different authorities.

Some of the figures of Dr. Bastian, I confess, astonished me. To judge from the drawings and statements it would appear that a solution of tartrate of ammonia and phosphate of soda *in vacuo*, was a much more potent generator of life than a solution of boiled turnip or boiled hay. I was certainly very much surprised when I saw Figs. 11, 12, 13, 14, and 15. If the organisms represented in those drawings have been produced, as Dr. Bastian states, in saline solutions destitute of living germs *in vacuo*, the fact is, indeed, so very important and of such very great interest, that it would have been better, in my opinion, to have directed attention to it alone without mixing up the question of fact with Mr. Herbert Spencer's probabilities. The experiments are either sound or not. If the former they do not require the support or assistance of any *à priori* reasoning; if the latter, all the philosophical arguments that can be adduced will not procure their acceptance. Dr. Bastian could not have published his drawings without feeling that those at all familiar with such inquiries would be surprised—and those who support him must therefore not feel hurt at the reserve of "opponents," and because they do not write to the very next number of NATURE to declare themselves *convinced*, and ready to subscribe to the spontaneous generation doctrine and give it all the support in their power.

But indisposition on the part of many of us to accept immediately the inferences does not detract in the slightest degree from the acknowledgment of their importance. I for one, as I said before, am ready to accept them, but not yet, because, as far as I know, Dr. Bastian's results are exceptional, and the experiments may require repetition. Nor could I thus early make the inquiries that would be necessary concerning the details and the many little precautionary measures that are requisite, and which, for aught that appears, have been taken, without seeming to be offensive. But I cannot help saying here that many of the preliminary remarks, and many observations in the notes, and much of the argument, are calculated rather to prejudice the mind against the conclusions sought to be established by experiment.

All the arguments hitherto adduced in favour of spontaneous generation fail to convince. Neither Mr. Herbert Spencer nor Mr. J. S. Mill himself could convince any man acquainted with the facts of the case, that heterogenesis really occurs in these days. At the same time a very strong case—nay, a case certain to convince anyone who was not practically acquainted with the matter—might be made. Such a case has indeed many times been made, and has produced a vast number of converts, some of whom have afterwards fallen away from the faith, and have been led to believe in another view. Many things *seem* to be proved by irrefragable reasoning which are *not proved*, and cannot be proved in the present state of our knowledge. Dr. Bastian of course admits all this, or he would never have tried *new experiments*. It is therefore only these new experiments which, by adding to our knowledge, can alter the question as it stands. All the arguments concerning invisible germs of crystals, the mode of "building together the molecules of corn," "collocations," "*à priori* presumptions in favour of new modes of evolution," &c., &c., merely increase the difficulty experienced by ordinary mortals of grasping the real question at issue and discussing the theory within any reasonable limits. The introduction of these preliminary *à priori* considerations is calculated to confuse. They certainly interfere with the due concentration of the attention upon the results of the experiments. They will not excite persons who are at all conversant with the inquiry to put more trust in the new experimental results than they would be inclined to do without them. On me, I regret to say, they have a contrary effect. The fact of *à priori* arguments having been so very much dwelt upon, makes me think that the mind of the experimenter may have been to some extent prejudiced (prepossessed) in favour of the doctrine he seeks to support by new facts, and in this way they are calculated to excite in my mind, however much I may resist, a doubt whether the inferences which have been arrived at really have been deduced from facts of observation and experiment *only*.

Prof. Wanklyn, whose observations have called forth these remarks, is rather damaging the cause he hopes to serve in another way. He states that "rather more than one pint of average atmospheric air does not contain so much organic nitrogenous matter as corresponds to a cube of dry albumen of the $\frac{1}{250}$th part of an inch in diameter," and affirms that this quantity is altogether inadequate to account for the "immense multitudes of germs, the existence of which in atmospheric air is assumed by the vitalists." Now these "vitalists" hardly require "immense" multitudes, and instead of being "assumed," the presence of quite a sufficient number has been *proved*, and moreover, they can be seen by anyone who chooses to take the trouble to look for them.

But I thank Prof. Wanklyn for his fact and calculation. The evidence he adduces is very interesting, and conclusively in favour of the presence of living germs in the air; for those who accept the views he objects to, do not require a tithe or even a hundredth part of the albuminous matter he finds actually exists. The space occupied by a moist blood corpuscle $\frac{1}{3000}$ of an inch in diameter, which only contained even less than a *tenth of its weight* of dry albuminous matter, would be large enough to hold

so many living germs that if uniformly diffused through the pint of air, not a thimbleful could be taken without containing several. These germs, placed under favourable circumstances, would soon grow large enough to be detected without any difficulty.

LIONEL S. BEALE

The Source of Solar Energy

IT is, I think, rather unfortunate that Mr. Proctor, in his recent work entitled "More Worlds than One," should have re-advocated the earlier and now discarded views of Sir W. Thomson concerning the source of solar heat or energy by *meteoric percussion*. That theory, however ingenious as advanced by the physicist, is surely hardly one to be admitted by the astronomer. Nothing less than an intense desire or necessity for finding some solution to the problem, whence or how the solar heat is maintained, could have encouraged scientific men seriously to advance or support so plausible and unsatisfactory a doctrine, or one, when examined, so little supported by what we really know either of meteors or of nature's laws. Having given much attention to *meteoric* astronomy, may I be permitted briefly to state what I hold are serious and practical objections to the validity of the meteoric or dynamical theory as applied to the conservation of solar heat and energy.

1. Because meteors and aërolites are known to impinge and strike the earth in her orbit, *ergo*, as I understand Mr. Proctor, numbers infinitely greater must no doubt be constantly rushing into the sun, as a body at once far larger, and much nearer to myriads of such bodies than the earth herself; but which, at a much smaller distance, are more likely to be drawn into the sun. Now, all that we really do know about meteors amounts to this, that by far the greater number of shooting stars visible in our atmosphere, in size no larger than a bean, and really separated from each other by thousands of miles, belong to fixed and definite systems or rings, having fixed radiant points for certain epochs or periods, showing clearly that these bodies are revolving round the sun, in courses as true and regular as the planets themselves, and are no more eddying or rushing into the sun, merely because they are so insignificant, than is the earth herself. Having projected upon celestial charts the apparent courses or tracks of nearly 5,000 meteors, observed during every part of the year, I feel I am justified in stating that not more than seven or eight per cent. of the shooting stars observed on any clear night throughout the year, are *sporadic*, or do not belong to meteor systems at present known to us. More than one hundred meteor systems are now recognised, several of which appear most certainly to be connected with known comets; and from a paper I have just received from Professor Schiaparelli, of Milan, it would appear that the approximate average *perihelion* distance for 44 of these meteor systems is not less than 0·7, the earth's distance from the sun being 1·0; whilst of these 44 systems, only 4, or about 10 per cent. have their *perihelion* distance under 0·1, that is, approach the sun nearer than nine millions of miles! Now, it is pretty well admitted that meteors are intimately connected with comet systems, yet out of some 200 comets, the elements of whose orbits have been calculated with tolerable precision, only 5 per cent. have their *perihelion* distance under 0·1. The same argument holds good also for planets, whose numbers also diminish after a certain considerable mean distance from the sun. Are these facts, then, in accordance with the notion that meteoric bodies either increase in number as we approach the sun, or that meteors are so constantly losing their senses, or sense of gravity, as to be ever rushing into or against the sun? I might almost ask, do *any* meteors rush or fall into the sun? Is it probable that the *mass* of all "the countless myriads of meteors" in the solar system exceeds that of a single planet? whether that of Mercury or Jupiter does not much signify. When we take into consideration the gigantic amount of meteoric deposits required to maintain the solar heat for hundreds of millions of years, in the meteoric theory, surely the supply of meteors would long since have been exhausted, were the supply at least confined merely to the meteors under a mean distance of 0·1 belonging to our own solar system! The argument, to begin with, is in a great degree fallacious, *e.g.*, because meteors frequently strike the earth, they must, it is argued, strike the sun in vastly greater numbers, and with far greater velocities. But it is forgotten that the meteors themselves, like the earth, are revolving round the sun as a common centre, in regular orbits, and only by accident, as it were, come into mutual collision, just as the tail of a comet might pass through the system of Jupiter and his satellites; while to the end of time neither the earth nor the meteors need necessarily come into contact with the sun.

2. But it is not merely meteors belonging to the solar system which are taxed to provide fuel for our sun; *space* itself may be filled with meteors ready to impinge upon the sun. The arguments against this are: (1) judging from analogy as well as from facts, comparatively few meteors are *sporadic*, consequently the majority cannot belong to stellar space, but to our own system; (2) granting that space itself is really more or less filled with meteors, these would not necessarily rush straight into the sun, unless, as would very unlikely be the case, they had no proper motion of their own. They might be drawn into or enter our system, it is true, but, according to Schiaparelli, only to circulate like comets in definite orbits.

3. The *zodiacal light* is another victim to the emergencies of the *meteoric* theory of solar energy. Whethre composed of myriads of small meteors, or merely a nebulous appendage, or atmospheric emanation belonging to the sun, is it credible that for hundreds of millions of years there could, physically speaking, be sufficient material in the zodiacal light to maintain the sun's heat and supply all the fuel required? Has it ever yet been proved that the entire mass of matter constituting the zodiacal light, is either composed of matter in a solid state, or, if it were, that its mass would be equal to that of our own earth? If composed of separate meteors, are they not each individually revolving round the sun, rather than occupied in being gradually drawn into it as a vortex?*

Of course I do not say that meteors are *never* drawn into the sun, or that they may not occasionally and by accident enter the solar atmosphere; I have merely endeavoured to show that, from what we really do know about meteors and the laws of nature, it is highly improbable that our sun could derive, in sufficient quantity, a needful supply of fuel from meteoric sources. The comet of 1843, which approached the sun within 550,000 miles, was not sensibly deflected from its course; it is just possible that so small a thing as an aërolite might at that distance have been drawn into the sun; but is it not also possible, from what we know of comet and meteor systems, it may be wisely ordained that the smaller bodies of our solar system, such as meteors, do not as a rule approach the sun too closely; and they probably do not, if their *perihelia* distances are rarely under 10,000,000 of miles?

Aërolites are doubtless of larger size and weight than shooting stars, and, as far as is yet known, not so regular in their appearance as shooting stars; but even with that class of phenomena, we notice a certain degree of periodicity in *maxima* and *minima* for certain times of the year, tending to show that they also may be subject to regular laws, and not fall so frequently or promiscuously upon the sun's surface as has been sometimes supposed. If they do not fall in vastly greater numbers, area for area, upon the sun than they do upon our earth, certainly the dynamical effect would be very minute! I may here also observe that even these bodies generally fall to the earth without being consumed, and with a very moderate velocity; their original cosmical velocity having been lost before reaching the surface of the earth. In the case of an aërolite falling upon the sun's surface, its original velocity may similarly have been gradually checked in its passage through the solar atmosphere, and a considerable amount therefore of the calculated mechanical effect lost. Small meteors would probably be consumed thousands of miles from the real body of the sun, seeing that the sun's inflamed atmosphere is now known to extend at times some 50,000 miles. It might almost be a question whether the sun's proper heat may not even be greater than that caused by the simple friction of a meteor through the solar atmosphere!

I merely allude to these minor matters, however, in order to point out some of the numerous uncertainties and difficulties connected with this meteoric or mechanical theory of the origin and conservation of solar heat, in addition to those already alluded to, bearing more especially upon the astronomical bearings of the question. For the present it must still remain a mystery, whence or how the solar heat is maintained, or to what extent really wasted.

Prestwich, Manchester, July 11 ROBERT P. GREG

Choice of a Microscope

WITH all respect to the judgment of my friend Mr. Ray Lankester, I should like to be allowed to oppose a few of the statements made by him in his remarks on the Choice of a

* We beg to refer our readers to Jones' and Liais' observations of the Zodiacal Light. They certainly have not received the attention in this country that they deserve.—ED.

Microscope, in NATURE, No. 37. It is, I believe, quite a mistake to say that you cannot get a cheap working English instrument. The model of Crouch, of London Wall, for instance, is not very much dearer than Hartnack's small model, and yet, at the same time, is in every way better and more comfortable to work with. Crouch's rackwork is so good that for ¼ths and ⅛ths there is no need to resort to the fine adjustment, except on special occasions, whereas, with the sliding tube, the fine adjustment is so continually used that it is very soon thrown wrong, to say nothing of the trouble which a beginner has in working the sliding tube successfully. I once had some of Nachet's instruments, to which Hartnack's are very like, in use in my class at University College, and the sliding movement was very successful in smashing my best specimens and injuring the front surfaces of my lenses. Then again, the English length of tube is undoubtedly an advantage for a slanting position of the microscope, and I suppose that is by far the most common position in which an instrument is used. In addition, Crouch's instrument affords an admirably effective but simple stage movement, which may be entirely removed at pleasure, and has a simple sub-stage tube, into which, if required (and in London it often becomes a necessity), a condenser might be fitted. Crouch's instrument is like Hartnack's, simple but strong, steady, and cheap; it differs in being about three times as convenient, and will, probably, last twice as long.

Nor can I agree with Mr. Lankester in recommending the general purchase of Hartnack's glasses for student's use. His No. 8, for instance, in many respects an admirable glass, is terribly close, and for this, and apparently for other reasons, very soon gets spoilt when used by students. I have been using lately for my classes at University College, a ⅛th of Crouch's, which, as far as ordinary histological work is concerned, performs in the most satisfactory manner, and yet is quite a cheap glass. I suppose the question of the price of labour prevents our English manufacturers from bringing down their prices to quite the French level, but I believe we get quite an equivalent for the slight excess in the form of greater convenience and better workmanship.

M. FOSTER

Colour-Blindness

THE nature of colour-blindness has never, I think, been satisfactorily ascertained. The usual explanation appears to be that the eye of the colour-blind is insensible, or nearly insensible to light of some particular colour. This I regard as in many respects unsatisfactory, and as I am not aware that the theory which I now suggest has been advocated before, I venture to lay it before the public.

There are no doubt some cases in which the eye seems partially insensible to particular colours or to colours in general. In such cases, however, I believe there is usually defective vision, and not proper colour-blindness. Those only are to be regarded as truly colour-blind who can perceive figures distinctly, but confound colours which other persons distinguish. Such was the case, for example, with Dugald Stewart, who could not distinguish between the colour of the leaves and the fruit of a Siberian crab; but he saw both, and therefore could distinguish the colour of both from that of the sky or cloud which lay beyond them. I mention this case more especially because Stewart was a psychologist, and maintained in opposition to Reid that variety of colour is the means by which we perceive visible figure. This is at least conclusive as to the perception of variety of colours by the colour-blind, which all observations made upon them point to.

Many philosophers have attempted to explain the phenomenon by assuming an insensibility to some colour, red for example. My reasons for rejecting this explanation are: (1) that in some cases where the experiment was tried (see Prof. Wartmann's paper in the Scientific Memoirs for November 1844) the colour-blind person saw the whole of the visible spectrum; and (2) that if red (for example) were seen as black, there would be no danger of confounding it with green, which would, on this hypothesis, be the colour seen most distinctly; but, in fact, confusion in regard to one colour almost always extends to the complementary tint.

The explanation I would offer is that derived from seeing accidental or complementary colours. It is a known fact that the eye has, in general (whether natural or acquired), a peculiar aptitude for white light, and thus if I gaze on a bright surface of any other colour, and look away rapidly towards a dark ground, the complementary hue becomes immediately visible. Nor can it, I think, be doubted that the complementary hue is not produced by the act of looking away. Green, for example, is produced by the red light falling on the eye, and the effect of looking away towards the dark ground is merely to make the green separately visible by cutting off the supply of red. It previously coexisted with and modified the red, but in ordinary eyes only to such an extent as not to prevent the red from strongly predominating in the total perception. This coexistence of the complementary colour with that actually visible is, I believe, known to persons accustomed to make delicate experiments in optics. I recollect in some lectures on the subject which I attended two or three years ago (where the equality of two lights of slightly different colours had to be determined), the professor cautioned us against looking too long at the lights, as he always found in his own case that there was a change of shade and a consequent impairment of the accuracy of his determination if he did so. This I have no doubt arose from the cause I have intimated. When we bear in mind the mutual excitation of sound-vibrations, the fact will create no astonishment. Now I apprehend that in most instances true colour-blindness arises from these complementary colours being excited more rapidly and with greater intensity than in ordinary eyes. If, for example, on looking at a red object the complementary green was excited almost at once, and with such intensity as materially to modify the red; and if, on the other hand, on looking at a green object, the excitation of the complementary red took place with equal readiness and intensity, it is clear that such an eye could not distinguish red from green. Both colours would, in fact, be seen after the first instant as a white or grey. In confirmation of this view I may remark that, according to Seebeck, all the colour-blind persons whom he examined confounded the colours with grey. Another argument in its favour is, that a confusion in regard to one colour seems (according to Wartmann) always to extend to the complementary tint. Again, it is natural to suppose that the production of complementary colours will take place rapidly and with considerable intensity when the eye is unusually sensitive to the incident light. Now, I find this unusual sensitiveness noticed in several of Wartmann's examples. One young woman could read for nearly a quarter of an hour (in the evening) after any one else could. In the cases mentioned by Goethe, the sight of the young men was "very good," and they "appreciated with great delicacy the gradations of light and dark." "Many Daltonians," says Wartmann, "see better in a demi-obscurity than other persons whose sight is more piercing by day than theirs," which he goes on to say was the case with three whom he himself had examined. Lastly, from the same paper it would appear that the colour-blind are either insensible to the phenomenon of accidental (complemental) colours, or see it with great difficulty. This, of course, is just as it should be on my theory. The colour-blind man sees both colours while looking at the coloured object, and he will again see both on looking away from it at the dark ground. If, for example, the colour looked at be red, the accidental green is seen while looking at the red, and it is also vivid enough to produce a secondary accidental red on looking away. The change produced by looking away will, therefore, be very slight, and hardly, if at all, perceptible.

I do not, however, put this theory forward as a complete explanation even of true colour-blindness. In addition to accidental or complemental colours, I believe there is often another phenomenon which may be called subjective colours, which modifies the total perception. In jaundice, it is well known, that black objects will appear yellow, and Dr. Wartmann records one case in which black appeared to the eye of the patient as green or crimson. I may have something more to say on this point hereafter. In such eyes, in fact, the adaptation is not for white light, but for light of some other colour, and the whole phenomenon of accidental colours is altered accordingly.

If these views be correct, it is evident that the colour-blind man will be best able to discriminate colours when he merely takes one glance at the coloured object, and then looks away towards a dark surface. This is worth trying. The fact that form is most easily discerned by taking a pretty long look at the object, makes a man follow the same course when he wishes to discriminate colours, but the advisability of doing so may be doubted. Another consequence is, that the colour-blind man would probably discriminate colours more readily in a faint light than in a bright one. These two observations can be easily made, and if my prediction should prove correct, the result will be of practical advantage to the colour-blind as well as a confirmation of the theory.

Trinity College, Dublin, July 9 W. H. S. MONCK

THE GUATTARI ATMOSPHERIC TELEGRAPH

THIS new invention is stated to consist of certain arrangements and combinations of apparatus whereby ordinary air compressed and passed through a tube, is utilised as a means of communicating intelligence from one given point to another, effecting the same object as the electric telegraph.

The principal portion of the apparatus consists of a reservoir or air-vessel which is charged or filled with air compressed to any desired degree according to the initial velocity or force which it is requisite the movements of the air employed should possess. A double action compression pump, or any other suitable mechanism, may be employed to charge the reservoir or air-vessel, and to sustain the pressure to the required degree. The reservoir or air-vessel is connected by means of a tube or pipe with a writing apparatus of any suitable description, and such as are well known and understood, especially in connection with electro-telegraphy; the tube or pipe being provided with a cock by which more or less force may be given to the current of air whereby the writing mechanism is actuated. In order to regulate the signals, a governor or piston, actuated by hand, is employed, by which pulsations or movements of the air in the tube or pipe are transmitted through a valve which is arranged therein, the currents actuating a lever connected with the writing apparatus. For the purpose of giving or receiving signals, the before-mentioned tube or pipe is connected with a conducting tube or pipe constructed of any suitable material, and which is so arranged that communication can be established between the air reservoir, or vessel, and the writing engine which is placed at the receiving station, or *vice versa*, by means of stop-cocks which are worked by hand. An indicator is employed to show the force of the current of air passing through the transmitting tube or pipe. Similar arrangements are, of course, placed and employed at each end of communication. By means of this invention it is stated that intelligence and signals can be transmitted to any distance; any of the known receiving and recording instruments capable of being used in connection therewith being employed. It is obvious that any number of conducting tubes may be employed, the requisite currents or pulsations of air therein being produced as before mentioned. The Guattari system claims to be more simple than the electric system, both in point of construction and continuous use, for whereas in the latter case it is necessary to use the electric battery and all its accessories, by the former ordinary atmospheric air compressed will perform similar functions. It is also claimed for it that it is free from atmospheric influences, which it is well known materially disturb the electric telegraph on the occasion of storms; and that the tubes employed as the medium for conducting the air would not be subjected to accidents like the ordinary wires, and would therefore necessarily last longer, and thus prove much more economical. We understand also that it is so simple that any person may learn in a few hours how to use and work it with the greatest ease, as compared with the electric system; it is calculated that the machinery necessary to work this system could be produced at about one-half the producing and annual working cost of the electric system.

The Royal Scientific Institute of Naples has already awarded to Signor Guattari a gold medal in recognition of what they consider an important invention, adding a graceful tribute on its presentation to the effect that it was the only gold medal which the Institute had ever awarded. The following experiments were made on Monday, 11th July, 1870:—

1. Transmission by atmospheric compression by means of the large machine, obtaining answers by impulsion and repulsion, Signor Guattari having at present but one machine.

2. System of impulsion and repulsion by a naval apparatus, which may be used with five different derivations or branches.

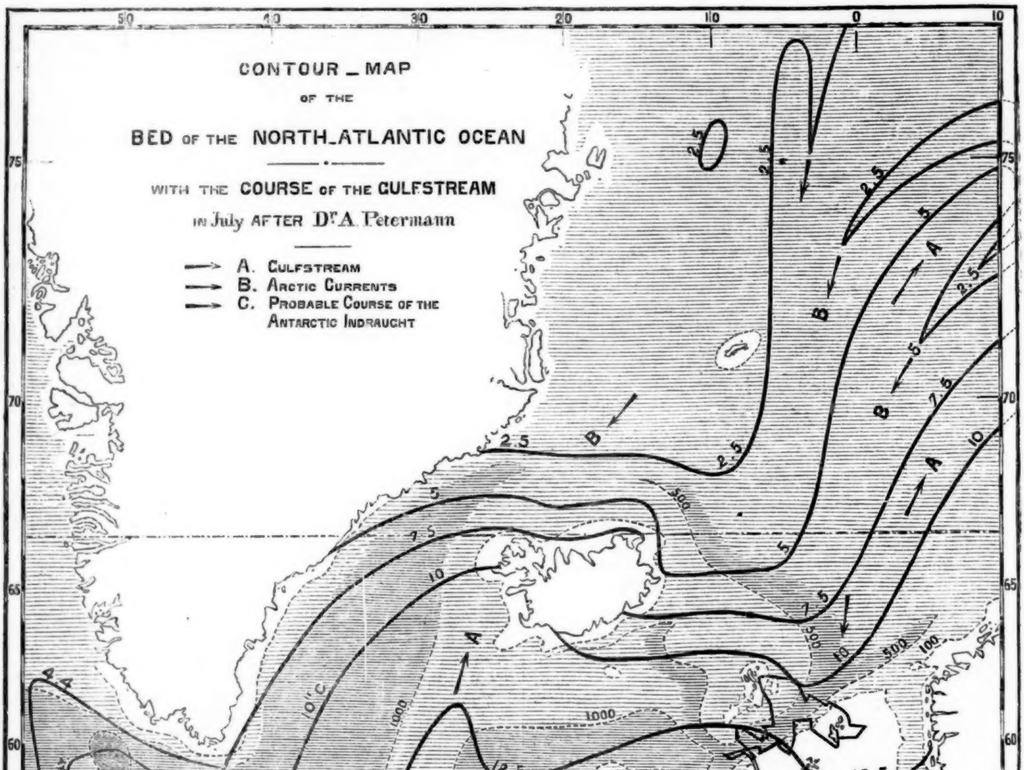
3. Universal telegraphy, namely, despatching telegrams to one or more stations at the same time without the aid of the transmitting machine or the necessity of the sender remaining fixed to any one point.

ON DEEP-SEA CLIMATES *

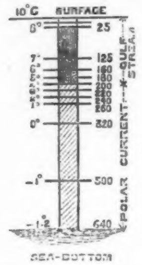
RECENT investigations have certainly tended to confirm the view originally advocated by my colleague, Dr. Carpenter, and myself, that a large portion of the bottom of the present sea has been under water and continuously accumulating sediment, at all events since the commencement of the "Cretaceous period," and possibly much earlier. The marked parallelism which, setting aside all local dislocations and denudations, evidently exists between the Jurassic, the Cretaceous, the Tertiary formations, and the present sea-board, and the evident relation of that parallelism to the older rock axes, would seem indeed to indicate that the main features of the present physical geography may date from a period even anterior to the deposition of the older Mesozoic rocks. With many minor and temporary oscillations, of which we have ample geological evidence, the borders of the Oolitic, the Cretaceous, and the Tertiary seas, have apparently been successively and permanently raised, and the ocean over an area, the long axis of which may probably correspond with that of the Atlantic, proportionally contracted. The question simply is, whether, since the elevation of the Jurassic beds, any oscillation has at any time raised into dry land the whole of the trough of the Atlantic, so as to arrest the deposit of sediment abruptly over the area, and to extinguish all animal life, thus defining what seems to be popularly understood as the close of a geological period, and requiring the complete repeopling of the succeeding sea by immigration, or, according to another view, by the creation of an entirely new fauna. It seemed to us on the whole more probable that the successive elevations of the borders of the Mesozoic sea were accompanied by compensating depression and deepening of the centre of the trough, which may thus have been inhabited throughout by a continuous succession of animal forms; at all events, the onus of proof appeared to rest with those who maintained any breach of continuity.

The deep-sea dredgings on both sides of the Atlantic have brought to light a very large number of hitherto unknown animal forms, and undoubtedly the assemblage bears a decided resemblance to the fauna of the chalk—a resemblance which increases as the investigation proceeds. Probably the most striking point is the apparent identity of the material of the chalk with the chalk-mud of the Atlantic; the globigerinæ and coccoliths by whose accumulation the beds have been, and are now, being produced, seem to be the same; though, of course, it is difficult to determine with certainty the specific identity of such simple and variable forms. Sponges are abundant in both, and the recent chalk-mud has yielded a large number of the examples of the group *porifera vitrea*, which find their nearest representatives among the ventriculites of the white chalk. From Prof. Martin Duncan's report it would appear that the corals, which are chiefly confined to water of moderate depth, are most nearly allied to those of the later Tertiaries. The echinoderm fauna of the deeper parts of the Atlantic basin is very characteristic, and yields an assemblage of forms which represents in a remarkable degree the corresponding group in the white chalk. Species of the genus *Cidaris* are numerous; some remarkable flexible forms of the Diademidæ seem to approach *Echinothuria*. M.

* The substance of a Lecture delivered to the Natural Science Class in Queen's College, Belfast, at the close of the summer session, July 15, 1870.

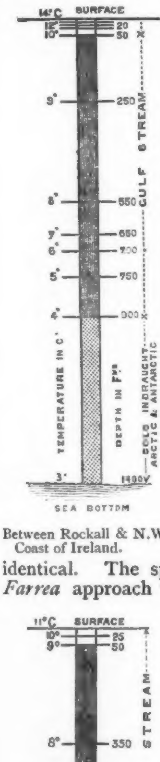

CONTOUR _ MAP
OF THE
BED OF THE NORTH_ATLANTIC OCEAN
WITH THE COURSE OF THE GULFSTREAM
IN July AFTER Dr A. Petermann
A. GULFSTREAM
B. ARCTIC CURRENTS
C. PROBABLE COURSE OF THE ANTARCTIC INDRAUGHT

de Pourtales dredging in the Gulf Stream in the Strait of Florida has found a true *Salenia*, several representatives of the chalk forms of *Cassidulidæ*; and M. de Pourtales on the American side, and we off the west coast of Ireland and off the Shetlands, have dredged a remarkable form, appropriately named by Lyman *Pourtalesia miranda*, which is most nearly related to the Jurassic and Cretaceous genus, *Dysaster*.



Between the Faröe and Shetland Islands. 61° 21′ N. Lat., 3° 44′ W.L., Gr.

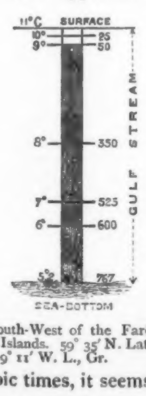
The Crustaceans of the chalk are, as yet, very imperfectly known, so that little can be founded upon them. The Mollusca have not yet been worked up, but the large number of smooth terebratulæ, and of species of the genera *Aporrhais*, *Dentalium*, *Pecten*, *Lima*, &c., from the deeper water, are highly suggestive of older times. Mollusca are, of course, most abundant in comparatively shallow water, and we were prepared to hear from Mr. Gwyn Jeffreys that out of about 120 species new to the British area, dredged in the *Porcupine* expedition, a large number date back to the newer Tertiaries.



Between Rockall & N.W. Coast of Ireland.

With these facts before us, it can scarcely be a matter of surprise that the point of view of those who are carrying on these investigations is insensibly changing, and that when the dredge comes up from a depth of one or two thousand fathoms the number of new species which it may contain is not now so much the question as the relation which these new forms may bear to their ancestors of an earlier epoch, and the light which they may be expected to throw upon types hitherto supposed to be entirely extinct.

Although there is so striking a resemblance in general character between the fauna of the European chalk and that of the deeper portion of the bed of the Atlantic, especially of a band extending from a depth of 400 fathoms to 900 fathoms in the Gulf Stream area, none of the animals, with the possible exception of some of the Foraminifera, are absolutely identical. The species of *Sympagella*, *Holtenia*, and *Farrea* approach the siphonias and ventriculites very nearly, but they form a distinct subsection of the order. *Rhizocrinus* and its allies resemble *Bourgetticrinus*, and undoubtedly represent it, but there are important differences. The Saleniæ, the Cassidulidæ, and the Dysasters, &c., of the chalk mud approach their Cretaceous antetypes more closely than they do any known living forms, but they are generally dwarfed, and otherwise diverge so far as to require in most cases the establishment of new genera for their accommodation. Now, if we admit the continuous accumulation of sediment of the same character, and the persistence of the same general conditions over a large portion of the area of the present ocean from Mesozoic times, it seems at first sight more difficult to account for this great amount of modification than for the perpetual recurrence in deep dredgings of forms suggestive of close relationship to, and lineal descent from, extinct species.



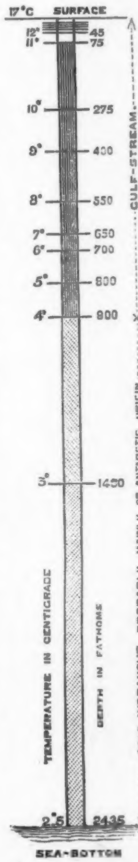
South-West of the Faröe Islands. 59° 35′ N. Lat., 9° 11′ W. L., Gr.

At the bottom of the ocean, where other conditions are comparatively uniform, we may probably regard successive changes of temperature as the main cause of successive alterations in the fauna of a region, by the modification, extinction, emigration, and immigration of species. It is my object, in the present lecture, to show that in the vertical oscillations which are known to have occurred since the close of the Mesozoic period, we have a *vera causa* of alternations of temperature fully adequate to the entire result.

In order to understand this point thoroughly it will be necessary, in the first place, to pass in review the present conditions of distribution of temperature in the North Atlantic.*

There seems to be little room for reasonable doubt that the present temperature of the basin of the North Atlantic depends, it may be said, *entirely*—for other modifying causes, such as the drift of the variable winds and the surface-heating and consequent expansion of equatorial water, are comparatively trifling—upon the Gulf Stream and the general indraught of cold water from the Arctic and Antarctic basins to supply the place of the constant warm current streaming north-eastwards from the Strait of Florida. Means of summer temperature which indicate roughly, not quite exactly, for higher temperatures, the mean amount of heat derived from the sun by direct radiation: the heat derived from all other sources, have been reduced from many thousands of isolated observations, and their results incorporated in an admirable and careful paper by Dr. A. Petermann (Geographische Mittheilungen, 1870).

The curves on the accompanying map, copied from Petermann, explain at a glance the distribution of abnormal temperature along the coasts of Western Europe, and indicate unmistakeably the source and direction of the warm current. One point only remained in doubt, namely, the depth to which the temperature of the ocean is affected by the Gulf Stream water. Now that there has been time to correlate and compare the large series of invaluable observations made with consummate skill and care by Captain Calver, R.N., during the *Porcupine* Expedition, this question may be considered solved over a considerable area, and the depth of the Gulf Stream off the west coast of Great Britain and France determined at about 800 fathoms (4,800 feet). This is so very im-



Bay of Biscay. 47° 38′ N Lat., 12° 8′ W. L., Gr.

* My colleague, Dr. Carpenter, in many interesting communications on the temperature results of the *Porcupine* expedition (NATURE, Vol. i., p. 490, &c., &c.), denies that the Gulf Stream exercises any influence upon the temperature of the basin of the North Atlantic, and doubts whether it reaches the coast of Europe at all. He attributes the differences of temperature between different zones of depth to "a great general movement of equatorial water towards the polar area, of which movement the Gulf Stream contributes a peculiar case, modified by local conditions." And if I understand him aright, he supposes that this general movement is produced by some cause analogous to that which produces the general circulation in the atmosphere. I am sorry to be obliged to dissent so completely from his view on this point.

portant a matter in connection with the distribution of animal life and the other conditions of the problem at present more especially before us, and there are still so many wide differences of opinion with regard to it, even among competent authorities, that, at the risk of repeating a good deal that you know already, I will explain to you as simply as I can what appears to me to be the present state of knowledge with regard to it. This must be considered, however, merely an outline sketch, and when a phenomenon is represented as a sole cause or a sole result, I mean simply to convey that it is a cause or result so paramount as to reduce all accessories to insignificance.

The ultimate source of the Gulf Stream is undoubtedly, as has been specially insisted upon by Sir John Herschel, the equatorial current of the Atlantic, the drift of the trade winds. The path of that portion which trends north-eastwards is determined by the great initial velocity of the equatorial water which escapes from the Strait of Florida. The glory of the Gulf Stream, as it issues from the Strait, has been the theme of every physical geographer; and Mr. James Croll, in a valuable paper in the February number of the *Philosophical Magazine* on Ocean Currents, has entered into a careful examination of the actual amount of heat conveyed by the Gulf Stream from the Tropics into Temperate and Arctic regions. Mr. Croll calculates the Gulf Stream as equal to a stream of water fifty miles broad and 1,000 feet deep flowing at a rate of four miles an hour, consequently conveying 5,575,680,000,000 cubic feet of water per hour, or 133,816,320,000,000 cubic feet per day.

This mass of water has a mean temperature of 18° C. as it passes out of the Gulf, and on its northern journey it is cooled down to 4·5°, thus losing heat to the amount of 13·5° C. The total quantity of heat, therefore, transferred from the equatorial regions per day amounts to something like 154,959,300,000,000,000,000 foot-pounds.

This is nearly equal to the whole of the heat received from the sun by the Arctic regions, and reduced by a half to avoid all possibility of exaggeration, it is still equal to one-fifth of the whole amount of heat received from the sun by the entire area of the North Atlantic.

The basin of the North Atlantic forms a kind of *cul-de-sac*, and while a large portion of the Gulf Stream water, finding no free outlet towards the north-east, turns southwards at the Azores, the remainder, instead of thinning off, has rather a tendency to accumulate in the northern portions of the trough. We accordingly find that it has a depth off the west coast of Ireland of at least 4,800 feet, with an unknown lateral extension. There are no data as yet to determine the rate of the branch of the Gulf Stream which sweeps round the coast of Western Europe and into the Arctic Sea, but it must be very slow, for even so far south as at lat. 42° N. it has lost all effect upon navigation, its character as a constant current being entirely masked on the surface by the drift of the anti-trades, which has nearly the same direction.

The Gulf Stream is thus a constant "river" of hot water, forced into a particular direction by the rotation of the earth, by the constant winds, and by the configuration of the land; and accumulated and modelled by the confined basin of the North Atlantic and Arctic Sea. The cold water which replaces it is supplied under very different conditions.

Sea water increases steadily in density as the temperature falls till it reaches its freezing point, about 3° C.; the coldest water, therefore, lies at the bottom, and if over any region warm water be removed by any cause from the surface, as for instance in the case of the equatorial current and the Gulf Stream, its place will be supplied by a general indraught beneath of water from the coldest and heaviest, and consequently usually from the deepest sources from which it can be brought in by gravitation. The cold water is, however, merely drawing in to supply a vacancy, and there is no special reason why it should follow one ingress rather than another. From the low initial velocity of polar water it will tend to flow westwards in passing into lower latitudes, but that tendency will probably be entirely subordinate to specific weight in determining the course of the cold influx and the distribution of layers of water of different temperatures.

As cold water can gravitate into the deeper parts of the ocean from all directions, it is only under peculiar circumstances that any movement having the character of a current will be induced; these circumstances occur, however, in the confined and contracted communication between the North Atlantic and the Arctic Sea. Between Cape Farewell and North Cape there are only two channels of any considerable depth, one very narrow along the east coast of Iceland, and the other along the east coast of Greenland. The shallow part of the sea is entirely occupied, at all events during summer, by the warm water of the Gulf Stream, except at one point, where a rapid current of cold water, very restricted and very shallow, sweeps round the south of Spitzbergen and then dips under the Gulf Stream water at the northern entrance of the German Ocean.

This cold flow, at first a current, finally a mere indraught, affects greatly the temperature of the North Sea; but it is entirely lost, for the slight current which is again produced by the great contraction at the Straits of Dover has a summer temperature of 7·5° C. The path of this cold indraught from Spitzbergen may be readily traced on the map by the depression in the surface isothermal lines; and in dredging by the abundance of gigantic amphipodous and isopodous Crustaceans and other well-known Arctic animal forms. The other two Arctic currents along the coasts of Iceland and Greenland are likewise very apparent, taking a slightly western direction from their low initial velocity.

But while the communication between the North Atlantic and the Arctic Sea itself, a second *cul-de-sac*, is so restricted, limiting the interchange of warm and cold water in the normal direction of the flow of the Gulf Stream, and causing the diversion of a large part of the stream to the southwards, the communication with the Antarctic basin is as open as the day, a continuous and wide valley of upwards of 2,000 fathoms in depth stretching northwards along the western coast of Africa and Europe.

That the southern cold water wells up into this valley there could be little doubt from the form of the ground, but here again we have curious corroborative evidence on the map in the remarkable reversal of the curves of the surface isotherms. The temperature of the bottom water at 1,230 fathoms off Rockall is 3·22° C., exactly the same as that of water at the same depth in the serial sounding, lat. 47° 38′ N., long. 12° 08′ W., in the Bay of Biscay, which affords a strong presumption that the water in both cases is derived from the same source; and the bottom water off Rockall is warmer than the bottom water in the Bay of Biscay (2° 5′ C), while a cordon of temperature soundings drawn from the north-west of Scotland to a point on the Iceland shallow, gives no temperature lower than 6·5° C. This entirely precludes the idea that the low temperature of the bottom water of the Bay of Biscay is due to any portion of the Spitzbergen current passing down the west coast of Scotland; and as the cold current of the east of Iceland passes southwards considerably farther to the westward, as indicated on the map by the successive depressions in the surface isotherms, the balance of probability seems to be in favour of the view that the conditions of temperature and the slow movement of this vast mass of moderately cold water, nearly two statute miles in depth, are to be referred to an Antarctic rather than to an Arctic origin.

The water of the North Atlantic thus consists first of a great sheet of warm water, the general northerly reflux of the equatorial current, the most marked portion of it

passing through the Strait of Florida, and the whole generally called the Gulf Stream, of varying depth, but attaining off the west coast of Ireland and Spain a depth of 800 to 900 fathoms. Secondly, of a general indraught of Antarctic water compensating at all events that part of the Gulf Stream which is deflected southwards; and thirdly, of a comparatively small quantity of Arctic water which, flowing through two or three narrow channels, replaces that portion of the Gulf Stream which makes its way into the Arctic Sea. As I have already said, the Gulf Stream loses an enormous amount of heat in its northern tour. At, say a point 200 miles west of Ushant, where the observations at the greatest depth were made on board the *Porcupine*, a section of the water of the Atlantic shows three surfaces, at which interchange of temperature is taking place. 1. The surface of the sea, that is to say, upper surface of the Gulf Stream layer, is losing heat rapidly, *a* by radiation, *b* by contact with a layer of air, which is in constant motion, and perpetually being cooled by convection; and *c* by the conversion of water into vapour. As the cooling of the Gulf Stream layer takes place principally at the surface, the temperature of the mass is kept pretty uniform by convection. 2. The band of contact of the lower surface of the Gulf Stream water with the upper surface of the water of the cold indraught. Here the interchange of temperature must be very slow, though that it does take place is shown by the slight depression of the surface isotherms over the principal paths of the indraught. The cold water being below, convection in the ordinary sense cannot occur, and interchange of temperature must depend upon conduction and diffusion, causes which in the case of masses of water of such depth must be almost secular in their action, and probably to a much greater extent upon mixture produced by local currents and by the tides. 3. The third surface is that of contact between the cold indraught and the bottom of the sea. The temperature of the surface of the earth is calculated at about 11°C., but it would be completely cooled down by anything like a movement and constant renewal of cold water; all we can say, therefore, is that contact with the bottom can never be a source of depression of temperature. As a general result, the Gulf Stream water is nearly uniform in temperature throughout the greater part of its depth; there is a marked zone of intermixture at the junction between the warm water and the cold, and the water of the cold indraught is regularly stratified by gravitation; so that in deep water the contour lines of the sea bottom are, speaking generally, lines of equal temperature. Keeping in view the enormous influence which ocean currents exercise in the distribution of climate at the present time, I think it is scarcely going too far to suppose that such currents, movement communicated to the water by constant winds, existed at all geological periods as the great means, I had almost said the sole means, of distributing heat in the ocean, and thus producing general oceanic circulation; they must have existed, in fact, wherever equatorial land interrupted the path of the drift of the trade winds. Wherever a warm current was deflected to north or south from the equatorial belt, a polar indraught crept in beneath to supply its place; and the ocean consequently consisted, as in the Atlantic and doubtless in the Pacific at the present day, of an upper warm stratum and a lower layer of cold water, becoming gradually colder with increasing depth. Wherever such conditions existed it is plain that mere vertical oscillations must have produced very decided changes of climate, through only a small number of degrees, but still very marked if the oscillation affected merely a portion of the cold underlying water, but enormous if it were sufficient to raise or depress the bottom of the sea, the principal theatre of animal life, so as to shift it from the cold layer into the warm, or from the warm layer into the cold.

One of the most striking phenomena connected with the distribution of heat in the North Atlantic is the case of the Shallow including the Hebrides, Orkney and Shetland Islands, and the Faroes, stretching westwards and northwards nearly to Iceland. The average depth is about 500 fathoms, and the Gulf Stream, which has a depth in these latitudes in summer of from 600 to 700 fathoms, occupies the whole of it, giving an abnormal temperature of something like 7° C. Owing to the peculiar conformation of the basin of the German Ocean, a tongue of cold water, with a bottom temperature of – 1° C. creeps into the valley between Scotland and Faroe, where it is overlaid by a stratum of Gulf Stream water, 150 fathoms thick. At the western mouth of the valley the cold water is banked in and retained by the water of the Gulf Stream, which is slowly passing the entrance of the gorge, giving a repetition, on a small scale, of the curious phenomenon described by Prof. Bache, off the coast of Massachusetts, as the "cold wall." My colleague, Dr. Carpenter, has conveniently called these two neighbouring districts, where the thermometer indicates 7°C. and – 1° C. respectively, the warm and cold areas. A depression, affecting that region of 250 fathoms equal to that which admitted of the accumulation of post-tertiary shells on Moel Tryfaen, would produce an extraordinary effect on its climate. In the first place, by mere subsidence, the Gulf Stream not reaching the bottom but flowing over a band of cold water, the temperature of the warm area would be reduced to, say, 3° C., and that of the cold, by an indraught of deeper water from the north, to – 2° C., but the Gulf Stream would no longer bank out the cold indraught from the north-east; which, in that case, passing down a deep open channel from the deep soundings to the west of the Loffotens, would spread along the bottom on the west coast of Scotland and Iceland, and greatly reduce its temperature, and probably entirely alter its fauna.

WYVILLE THOMSON

NOTES

WE have reason to believe that Professor Sir Wm. Thomson will be the next President of the British Association.

WE learn that the Royal Commission on Scientific Instruction and the Advancement of Science, which has met regularly two days a week, have now adjourned over the recess.

THE Royal Astronomical Society has issued a list of the members of the various learned Societies who propose to take part in the observations of the approaching total eclipse of the sun.

WE learn from the last number of the *Revue des Cours Scientifiques* which has reached us—that for the 24th inst.—that on Monday last week the Paris Academy of Sciences continued, in *comité secret*, the discussion of Mr. Darwin's nomination to fill the vacancy in the Zoological Section caused by the death of Purkinge. M. Milne-Edwards first spoke in his favour. While insisting on his own absolute opposition to evolutional doctrines, he rendered homage to the value of the special works of Mr. Darwin, especially the theory of the formation of coral islands. M. Elie de Beaumont also attested the value of this theory, and remarked that Mr. Darwin had done good work which he had spoiled by dangerous and unfounded speculations. He thought he should not be elected until he had renounced them. M. Emile Blanchard, who spoke for more than an hour, was very severe upon Mr. Darwin, styling him an "amateur intelligent," a remark capped by M. Elie de Beaumont (it is stated), who cried out, to the great indignation of M. de Quatrefages, "*C'est de la science mousseuse.*" M. de Quatrefages promised to answer M. Blanchard point by point on Monday last.

THE Archæological Congress is now in full swing at Leicester, one of the most interesting towns and localities which it is possible for such a Congress to visit. The proceedings commenced on Tuesday with an address presented by the Mayor and Corporation, followed by visits to some of the objects of interest in the

town. On Wednesday the section met at the usual time, and the Abbey was subsequently visited. To-day (Thursday) there are excursions to Ashby-de-la-Zouch, Tutbury, Tamworth, and Polesworth, and a *conversazione* in the evening. The future work is as follows:—Friday—Meeting of members for business; meetings of sections; excursion to Kirby Muxloe Castle; *conversazione* in the evening. Saturday—Excursion to Groby; Bradgate Park, Ulverscroft Priory, Woodhouse Chapel, Beaumanor Park, and Grace Dieu. Monday—Meetings of sections; excursion to Melton Mowbray and Oakham; *conversazione* in the evening. Tuesday—Meetings of sections; general concluding meeting.

PROFESSOR HELMHOLTZ will take up his new duties at Berlin in April, 1871.

THE Geological Survey of India, supported by the Government, have recently issued the completing part of the sixth volume of its "Memoirs." It contains a valuable paper by Mr. W. T. Blanford on the geology of the Taptee and Lower Nerbudda valleys in Western India, illustrated by maps, plates, and numerous woodcuts. Mr. Blanford describes the geology and physical geography of the country generally, giving first an account of the results obtained by previous observers, with references to their works, followed by more precise details of later observations, thereby furnishing an exceedingly valuable monograph. The view of a dyke at Koteda, near Goojree, exhibiting the trap in curved columnar masses, is a charming little picture. Mr. A. B. Wynne gives a description of some frog-beds exposed in Bombay Island in 1867. Numerous well-preserved skeletons of Oxyglossus pusillus (*Rana pusillarius*) were found associated with ribbed fragments of plants, and large, shapeless, structureless pieces of carbonised vegetable organisms. The skeletons were found in all kinds of postures: the hind legs extended, crossed, contracted, or twisted. Dr. Ferdinand Stoliczka, the palæontologist of the survey, gives a brief sketch of the osteology of the frogs discovered, with engravings of several specimens. The same gentleman, in two new parts of the "Palæontologia Indica," just issued, has given engravings and descriptions of some of the Gasteropoda of the Cretaceous rocks of southern India. This work, published at the expense of the Indian Government, is intended to comprise figures and descriptions of the organic remains found during the progress of the survey. The new parts complete the second volume of the Cretaceous fauna of India, with useful tables and an index of species. Mr. Thomas Oldham, the indefatigable superintendent of the survey, has commenced the publication of "Records" of the proceedings of the officers in various parts of India, of which the first four numbers are before us, containing much interesting geological information.

DEATH is hard on the medical profession just now. James Copland, M.D., F.R.S., died on the 17th inst., at the age of seventy-eight. From an obituary notice in the *British Medical Journal*, we learn that his principal claim to fame rests mainly on his performance of that gigantic work "The Medical Dictionary"—which he undertook, and carried to completion after a labour of thirty years. On finishing this work, he wrote in the preface that his labours, which had been incessant for many years, had been persisted in under circumstances and contingencies which few could have endured. Every line in it was written by his own hand, and all the proofs were carefully read and corrected by himself. The Dictionary is a cyclopædic *résumé* of all that has been written on the various subjects treated in it, from the earliest days of medicine down to modern times, with copious references to all the sources of information; and with all this are given the opinions which the author's observation and experience had led him to form. That one man should have undertaken and, labouring single-handed or nearly so, completed such a work, is indeed a remarkable fact. The work is a monument of calm energy and self-reliance, such as is but rarely met with.

NATURAL phenomena must be regarded by the engineer in the tropics. Here the boring worm will teach him salutary caution. In the East we have seen a railway train stopped on an incline by locusts. The locusts have a fancy for sitting on the rails, and when the engine-wheel touches them they are crushed, leaving the rails so oily that the engine slips. On one line, in the locust season, sand-boxes are used with the locomotive. Oysters are, however, a newly recorded enemy to the engineer. Some gourmand suggested the harbour of Tuticorin as a suitable place for oyster beds, and the Madras Government, doubtless appreciative of the value of oysters either for eating or for pearls, turned a deaf ear to remonstrance. Time has, however, justified the remonstrants, for, though the projectors have got an abundant supply of oysters, the harbour of Tuticorin is now said to be in danger of total destruction by the growth of the oyster beds, and the attention of the Government is seriously directed to cross the love of the oysters. The Madras coast is so ill-provided that harbours are more valuable than oysters, and a campaign will be directed against the latter, although the revenue authorities hanker after the taxes on the pearl fishery.

AN experiment, performed by M. J. M. Philipeaux, in which he transplanted one of the incisor teeth of a guinea-pig into the comb of a cock, has been referred to by M. P. Bert in his "Thesis on Animal Grafting," but M. Philipeaux thought that the examination of the specimen with an account of the experiment would be interesting to the members of the Society of Biology, and he accordingly submitted it to their inspection, with the following observations:—On January 13, 1853, M. Philipeaux, after having made an incision into the head of a young cock, introduced into it the incisor tooth of a guinea-pig that had been born a few hours previously. The tooth, very complete and furnished with its bulb, was so placed that the bulb was at the bottom of the wound and the extremity of the tooth turned outwards. On the day the experiment was made the tooth was eight millimetres long and two millimetres thick. The cock was killed ten months after the operation. The tooth, which, on the day of operation, was entirely concealed and covered in the wound, projected, when the animal was killed, five millimetres from the surface. M. Philipeaux had dissected out in the specimen the whole length of the tooth, and found that it measured no less than thirteen millimetres; it had consequently grown five millimetres. The interest of this experiment, which in other respects resembles those of Hunter and Sir A. Cooper, in which the spur of the cock was transplanted to the comb, consists in the circumstance that here a graft was accomplished in one animal of a part belonging to another, belonging to an entirely different zoological class.

WE are sorry to announce the death of Von Graefe, the most distinguished oculist in Europe. Iridectomy, the contribution to ophthalmic surgery with which his name is chiefly associated, was but one out of a multitude of operations which made his clinique at Berlin the resort of persons labouring under eye-diseases from all parts of the world.

WE have received from the Government of Victoria a most valuable collection of the Mineral Statistics of that Colony. Among the appendices is an important illustrated paper by Mr. Ulrich, F.G.S., entitled "Contributions to the Mineralogy of Victoria." We shall return, if space permits, to this interesting State paper. In the meantime, we may state that the total value of the gold raised in the Colony to the end of last year

has been 152,706,120*l.*, and that diamonds, rubies, sapphires, topaz, &c., are now being found.

WE learn from the *British Medical Journal* that M. Duruy (late Minister of Public Instruction), M. Nélaton (the great surgical celebrity of Paris), M. Husson (commonly called Sa Majesté l'Assistance Publique), and M. Milne-Edwards (the well-known semi-English Frenchman and philosopher), are at the head of a movement to found an *Ecole de Médecine* specially or women. The programme has been submitted to the Empress, accompanied by a request that she should become patron of the projected school. In France it is argued that there really is scope or something of this kind. It is alleged that the practice of medicine, from the present dull level of all being competent practitioners, is in a bad state; that it is necessary to institute a new body of practitioners, competent for drudgery and ordinary emergencies, but not competent for consulting practice. It is said that the mass of the profession are far too well educated, and that it is only by having a dash of the charlatan that even a good man can rise above the ranks. This surely is throwing a new light on the question.

THE Abbé Moigno announces in the last number of *Les Mondes* that the stone knives with which Joshua performed the rite of circumcision have been found in his tomb by the Abbé Richard. The account, which concludes as follows, we commend to the special attention of Mr. John Evans and Mr. Sorby:—"Voici donc qu'un des faits historiques les plus singuliers de la Bible a reçu la confirmation la plus éclatante, et que nous entrons en possession de silex taillés il y a 3,550 ans, plus vieux bien certainement, nous le prouverons jusqu'à l'évidence, (!) que les silex taillés de la vallée de la Somme ou des grottes d'Aurignac. Qui sait même si le spectroscope manié par des mains aussi habiles que celles de M. Sorby ne nous démontrera pas la présence, sur quelques-uns de ces silex, du sang de la circoncision." (!!)

OUR readers will recollect the important part played by the telegraph in the Seven Days' War, the introduction of this new arm, so to speak, enabling von Moltke to control all the strategical combinations with unerring accuracy, from a small room in Berlin. To the telegraph the Prussians have now added the balloon, and already we hear that the French army encamped in the environs of Metz have been surveyed with the greatest care. Surely if strategy is to play the part it did in former times in future battles, given two armies, one of which is, by means of a balloon, in electric communication with the Head-quarter Staff, in perfect knowledge of the numbers, at any one point, and the movements of the other, its success must be assured.

PERSONS wanting chemical and philosophical materials and apparatus from Germany, already experience a delay from the interrupted communications even in this beginning of hostilities.

THE heat has been so great at Dowlashéram, in the Madras Presidency, in June, that the Indian papers report many birds have died of sun-stroke.

AMONG the scientific labours of the Government in India in the departments of agriculture and horticulture, which have given such good results in tea and cinchona, may be recorded a garden at Raneekhet, the new hill station above Simla, for supplying vegetables to the troops, and an experimental cotton garden in the Boolundshur district. One chief object of the latter is to test the application of irrigation to various descriptions of cotton seed. Some appear to require water, and some to be injured, but there is no scientific record.

IN the progress of public opinion the public of Madras have come to the conclusion that the monkeys of that city, formerly held sacred, are a nuisance, and the municipality has taken measures to deport them. This requires—first, that they shall be caught. When caught they are to be tenderly treated; but for fear of their early return the aid of modern science is to be called in, and they are to be conveyed by railway trains to Tiruputty. In the distribution of animals the naturalist has thought fit to make little account of the railway, which may effect a displacement of the monkeys of India.

THE necessary operations for the construction of the sea-wall of the New Brighton Aquarium are progressing. It is expected that the aquarium will be completed and furnished by the spring of next year.

"SPONTANEOUS Generation and the Hypothesis of Physiological Units," by Herbert Spencer, is a reprint of a paper intended for the *North American Review*, in reply to an article that appeared in that journal entitled "Philosophical Biology," in which Mr. Spencer thinks that his views on the origin of life are not correctly represented.

THE *Scotsman* describes a Mirage in the Firth of Forth, the most extraordinary instance which can be remembered, which occurred on Friday afternoon. The day was very hot and sultry, and there was a peculiarity about the atmosphere which is seldom observed in this country. About midday a thin, clear, and transparent kind of vapour, through which the surrounding objects began to make their appearance in the most fantastic and grotesque shapes imaginable, settled over the sea. The phantasmagoria were principally confined to the mouth of the firth; but at one time they embraced the whole of the Fife coast as far as the eye could reach, town, village, and hamlet being depicted high up on the horizon with remarkable distinctness. Though the whole coast seemed at least half-way up the horizon, the appearances presented by the towns were very different, some of them having the houses inverted, while others appeared in the natural position. The Bass Rock, the Isle of May, and the rocks around Dunbar harbour, however, attracted most attention, both from their proximity and from the extraordinary forms which they assumed. The Bass, which at one time seemed to lie flat upon the sea, suddenly shot up into a tall spiral column, apparently ten times its usual height, surrounded by battlements rising tier on tier, and presenting a most imposing spectacle. As usual, however, the most fantastic appearances were presented by the May, which, in the course of the afternoon, underwent an almost innumerable series of phantasmagoric transformations. At one time it was apparently as round as a circle, at another seemingly draw out for miles against the horizon; now flat upon the water, then rising to ten times its usual height; occasionally portions appeared to break off and sail away, then to return and unite again—all within the space of a few minutes. Vessels in the offing appeared double—one on the water, and another inverted in the air; and in one instance three figures of one vessel were distinctly visible—one inverted, another on the sea, and a third in its natural position between the two. The fishing boats proceeding to sea in the evening underwent the same transformations when only a few yards off the shore, the double appearance being distinctly visible within a certain distance. The rocks at the harbour also seemed to play fantastic tricks, opening and shutting, rising and falling, with apparent regularity. These extraordinary illusions lasted from midday till night-fall, and excited great interest among the inhabitants of Dunbar, numbers of whom collected in the Castle Park and at the harbour for the purpose of witnessing the phenomena.

WE have received from M. J. L. Soubeiran an interesting account which he has communicated to the Société Impériale d'Acclimatation, of the progress of pisciculture in the Neilgherries, based on Mr. Day's paper in the Proceedings of the Zoological Society.

HOPKINS versus DELAUNAY

WE have received from Archdeacon Pratt a copy of a paper communicated to the *Philosophical Magazine* on Delaunay's objection to Hopkins's method of determining the thickness of the earth's crust by the precession and nutation of the earth's axis.

The archdeacon, on this most important question, states:—

"I am ready to allow, and so would Mr. Hopkins have allowed, that if the crust of the earth revolved round a steady axis, always parallel to itself in space, and if at some particular epoch a difference existed between the rate of movement of the crust and of the fluid within it, the resulting friction would gradually destroy this difference and bring about a conformity in the motion of both parts. I will even go further, and allow that the effect of the internal friction and viscidity of the fluid may be such that the resulting rotary motion may be the same as that which the whole mass would have had at the epoch if it had suddenly become one solid body and thereby suddenly retarded the rotation."

He thus illustrates his position:—

"Suppose a spherical shell or crust of mass C to have within it a solid spherical nucleus, of radius b and mass N, fitting it exactly; and the crust to receive an angular velocity of rotation around an axis fixed in the crust, the nucleus at that moment having no angular velocity; but suppose that a slight force of friction between the surfaces gradually generates a rotary motion in the nucleus; and suppose this force to vary as the difference between the angular velocities of the crust and nucleus—that is, of the surfaces in contact. Let ω and ω' be the angular velocities at the time t, k and k' the radii of gyration of the two bodies, F $(\omega-\omega')$ the force at the equator of the nucleus which represents the friction between it and the crust. Then the equations of motion are

$$\frac{d\omega}{dt}=-\frac{\mathrm{F}b}{\mathrm{C}k^2}(\omega-\omega'),\quad \frac{d\omega'}{dt}=\frac{\mathrm{F}b}{\mathrm{N}k'^2}(\omega-\omega')\ .\ \ .\ \ .\ \ (1)$$

Suppose also that β would have been the angular velocity, when the primitive impulse was given, on the hypothesis of the crust and nucleus being rigidly connected so as to be one mass. Then

$$\beta(\mathrm{C}k^2+\mathrm{N}k'^2)=\alpha\mathrm{C}k^2\ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ .\ \ (2)$$

Subtracting the second of equations (1) from the first, putting

$$\mathrm{F}b\left(\frac{1}{\mathrm{C}k^2}+\frac{1}{\mathrm{N}k'^2}\right)=c,\ .\ \ .\ \ .\ \ .\ \ .\ \ (3)$$

and integrating, we have

$$\omega-\omega'=\text{const.}\times e^{-ct}.$$

When $t=0$, $\omega=\alpha$ and $\omega'=0$;

$$\therefore\ \omega-\omega'=\alpha e^{-ct}.$$

Hence, by the first of equations (1),

$$\frac{d\omega}{dt}=-\frac{\mathrm{F}b\alpha}{\mathrm{C}k^2}e^{-ct}=-\frac{\mathrm{N}k'^2c\alpha}{\mathrm{C}k^2+\mathrm{N}k'^2}e^{-ct},\ \text{by (3)};$$

$$\therefore\ \omega=\alpha-\frac{\mathrm{N}k'^2}{\mathrm{C}k^2+\mathrm{N}k'^2}\alpha\left(1-e^{-ct}\right);$$

and also

$$=\beta\left(1+\frac{\mathrm{N}k'^2}{\mathrm{C}k^2}e^{-ct}\right),\ \text{by (2)}"$$

The paper then continues:—"The first of these expressions shows that the angular velocity of the crust begins with α; and when ct becomes very large indeed, it is reduced to β. Hence the effect of the constant friction of the nucleus against the inner surface of the crust is *at last* to reduce the velocity of the crust to what it would have been at first if the crust and nucleus had been one solid mass.

"We may conclude perhaps that the same effect would be produced, though in a much longer time, if the interior were not a solid sphere, but a fluid mass.

"The above reasoning shows that if the disturbing force producing precession and nutation did not exist, and the interior of the earth were fluid (whatever the thickness of the crust), it may be fairly assumed that the motion of rotation of the crust would now, the earth having existed so many ages, be exactly what it would have been had the earth been one solid mass, all difference of motion having been long ago annihilated by the internal friction and viscidity.

"But the disturbing force producing precession and nutation does exist. It consists of two parts, one constant and the other variable and periodical. The constant part is that which produces the steady precession of the axis (and which I will call for convenience the precessional force); the other produces the nutation. I will consider the precession first. Suppose now, for the sake of argument, that at the present moment, as M. Delaunay imagines, the crust and the fluid are revolving precisely as one mass, all previous differences of motion, even under the action of the disturbing force which produces precession and nutation, having been annihilated by friction and viscidity. I ask—What will be the action of the precessional force from this moment? It tends to draw the pole of the crust towards the pole of the ecliptic: and this tendency, as mathematical physicists well understand, combined with the rotary motion of the crust, produces this singular result, viz., the pole does not move towards the pole of the ecliptic, but shifts in a direction at right angles to the line joining the poles towards the west; so that the inclination of the axis to the ecliptic remains constant, but the axis shifts towards the west. The space through which it shifts in an infinitesimal portion of time varies as the length of the time and the force directly, and as the inertia of the mass to be moved inversely. The inertia of the mass depends upon the thickness of the crust only; for the friction of the fluid against the inner surface of the crust (which might, as I have shown, in the course of years, produce a sensible effect) cannot do so during the infinitesimal portion of time I am considering before the precession is actually produced. The precessional force has its full effect in producing the precession of the solid crust, the fluid not having time to diminish that effect before the axis has assumed a new position; and in this new position of the axis the precessional force is precisely the same in amount as before, to go on causing the precession as before. The precessional force is, in fact, ever alive and active, and shows this in incessantly producing the effect I have described; and the precession goes on steadily, the amount of it depending upon the mass of the crust thus moved, which the fluid has not time to retard or lessen. M. Delaunay says that 'the additional motion due to the above-mentioned causes (the disturbing forces which give rise to precession and nutation) is of such slowness, that the fluid mass which constitutes the interior of the globe must follow along with the crust which confines it, exactly as if the whole formed one solid mass throughout." In reply to this, I say that it is not the slowness of the motion, but the want of solid connection between the crust and the fluid in contact with it that affects the problem. The motion, whatever its amount, is incessantly being generated by the disturbing force, and owing to this want of solid connection, the friction of the fluid has not time during the successive moments during which the precession is generated, to stop or even sensibly to check it.

"It will thus be seen that at every instant the precessional force proceeding from the action of the sun and moon on the protuberant part of the earth's mass will, if the earth be a solid mass, have to move the whole mass; and if the earth have a solid crust only with a fluid interior, the force will have to move only the crust against the evanescent resistance of the fluid within during so short a space of time as it takes to produce precession. The resulting precessional motion will be different in the two cases; and therefore the actual amount of the precession which the earth's axis has (and which is a matter of observation) is a good test of the solidity or fluidity of the interior. This is Mr. Hopkins's method.

"The force producing nutation is much smaller, even at its maximum, than the precessional force. Its effect, however, is precisely the same in this respect—that it depends upon the mass of the solid crust, and in no respect upon the friction of the fluid within it, which has not time to influence the nutation before the nutation is actually produced.

"I do not here undertake to go into Mr. Hopkins's numerical calculations; I simply vindicate his method. I do not here consider what modification the elasticity of the solid material of the earth may have upon his numerical results. I conceive that it would have *no* effect, if the disturbing force were constant and there were no nutation. For, under the dragging influence (if I may so call it) of the constant precessional force, the solid material would be under a steady strain, and would communicate the effect of the force, continuously acting, from particle to particle of the solid part as if it were really rigid; and the resulting precessional motion would be greater or less as the mass of the solid part may be smaller or larger—that is, the solid crust thinner or thicker. But as the disturbing force is not constant, but variable, and there is constantly nutation of the axis as well as precession, the action above described will be somewhat modified;

and the elasticity of the solid material may be expected to have some influence on the result. This influence, however, will be minute, as the part of the disturbing force which is variable and produces nutation is very much smaller, even at its maximum, than the precessional force. The consideration of this matter, however, has no bearing upon the validity or not of Mr. Hopkins's method, but simply upon the numerical value of his final result, not upon the question of the fluidity or solidity of the earth's mass."

The Archdeacon is of opinion "that the strictures of M. Delaunay upon this method, which the genius of Mr. Hopkins devised, betray an oversight of the real point upon which the success of his method depends, and that this method stands unimpaired."

SCIENTIFIC SERIALS

THE *Geological Magazine* for July (No. 73) contains rather ewer original articles than usual, but what there are will be found interesting. The series of notices of eminent living geologists is continued in a notice of one of the most accomplished of the number, Professor John Phillips, of whom we have a good biography, but a very unsatisfactory portrait. Mr. Carruthers gives a notice of the so-called fossil forest near Cairo; he distinguishes a new species of *Nicolia* (*N. owenii*), and illustrates its microscopic structure as compared with that of the old species *N. ægyptiaca* Unger. — Mr. Kinahan communicates a paper containing a comparison of the geological features of Devon, Cornwall, and Galway, with a discussion of the means by which they have been produced; and Miss E. Hodgson a long disquisition on the origin and distribution of the granite-drift of the Furness district. The longest article in the journal is a report of Mr. David Forbes' lecture on Volcanoes, which will be read with much interest.

The *Journal of the Asiatic Society* for April, contains the following Natural History papers—Observations on some Indian and Malayan Amphibia and Reptilia, by Dr. F. Stoliczka. The species described in this paper have been partially collected by the author along the Burmese and Malayan coast, in Penang and Singapore, partially at the Nicobar and Andaman islands, only a few species are noticed from Java, and a few also from the N. W. Himalayas. Short notes on the geographical distribution, and on the general character of the amphibian and reptilian fauna of the Andamans and Nicobars, form a brief preface to the detailed descriptions. Complete lists of all the known species occurring on the two last-named groups of islands are appended. Dr. Stoliczka gave a short sketch of the relations existing between the Andaman and Nicobar reptilian fauna and that of Burma on the one, and that of Java, Sumatra, and the Philippine islands on the other hand. All these islands, he said, have many species in common. He also specially notices the very great number of viperine snakes (*Trimeresurus*) which are to be met with at the Nicobars, but fortunately these species appear to be less dangerous than continental forms usually are. The Nicobarese say that not a single fatal case has resulted from the bite of these *Trimeresurus*, and certainly all the specimens examined had a comparatively small poison-gland. The result of the bite is said to be only a swelling of the wounded part. Dr. Stoliczka also exhibited a specimen of the rare *Callophis intestinalis* obtained from Upper Burma. The species has the poison glands extending from the head to about one-third of the total length of the body, lying free in the cavity of the anterior part, and causing the heart to be much further removed backward than is generally the case in other species of snakes. The President thought there were one or two remarkable features in Dr. Stoliczka's interesting paper. One to which he particularly referred was the relative inefficiency of the poison in certain snakes of Penang and the Nicobars in comparison with the poison of the cognate species found in this country. He did not know whether the circumstances which rendered the possession of an invariably fatal weapon necessary to particular classes of snakes in the struggle for life, while others could maintain themselves without it, had yet received much attention. *A priori*, he thought, one would be disposed to expect that a poison which would disable without causing immediate death, would be more deterrent in its effects, and, therefore, more widely useful to its possessor, than one which killed instantly. At any rate it was curious to find some of the insular species of snakes, though provided with a perfect poison apparatus, much less fatal in the effect of their bite than other closely allied species in Bengal were. The investigation of the causes which had led to this difference ought to be attractive. A short discussion on the effects of snake poisoning ensued. Mr. Waldie desired to know what the symptoms resulted from the bite of the Nicobar vipers, and whether they are the same as are usually known to originate from the bite of other poisonous snakes. Dr. Stoliczka said that the Nicobarese only speak of a swelling of the bitten part, and that they exhibit very little fear of these snakes. Dr. Stoliczka also observed that the poison gland in the species of *Trimeresurus* which he had examined, has a simple glandular form without any appendages, but the skin forming it is very tough, and internally partitioned by numerous irregular lamellæ. The poison of the fresh snake was always present in a comparatively small quantity, and appeared less viscose than the Cobra poison. The differences between the effects of poisoning of the cobra and daboia had been pointed out by Dr. Fayrer.

SOCIETIES AND ACADEMIES

LONDON

Geological Society, June 22.—II. "On the Physics of Arctic Ice as explanatory of the Glacial Remains in Scotland." By Dr. Robert Brown, F.R.G.S., &c. In this paper the author entered into an extended inquiry how far the formation of the boulder-clays and other glacial remains in Scotland and the North of England can be accounted for, on the theory of a great ice-covering having at one time overlain the country in much the same manner as it does now Greenland and other extreme Arctic countries. Taking the hypothesis of Agassiz as his groundwork, Dr. Brown entered into a minute description of the present glacier-system of Greenland, and the nature of Arctic ice-action; and into an inquiry how far glacial remains in Britain correspond with those at present in course of formation in Greenland and at the bottom of Baffins Bay, Davis Straits, and the fjords and bays adjoining these seas. These inquiries were commenced in the year 1861, and have been continued at intervals ever since up to the present summer in various portions of the Arctic regions, the Continent of Europe, in Great Britain, and in North America across to the Pacific. The results of these extended researches have led him to conclude—1. That the subazoic boulder-clay corresponds with the *moraine profonde* which underlies glaciers, and in all likelihood is the immediate base on which the ice-cap of Greenland rests. 2. That the fossiliferous, laminated, or brick-clays find their counterpart in the thick impalpable mud which the subglacial streams are pouring into the sea, filling up the fjords, even shoaling the sea far out, and absolutely in some cases turning the glaciers from their course into other valleys. Allowing the very moderate computation that this impalpable mud accumulates at the rate of only six inches per annum, a deposit of fifty feet in a century must form. If Scotland was at one time covered with an ice-cap, or had glaciers of any extent (as cannot be doubted), then this deposit must have been equally forming, and as a geological formation must be accounted for. No difference could be detected between this glacial mud and the present brick-clays, and every fact went to show that it was to this that we must look for the formation of these laminated fossiliferous clays. The amount of earth deposited on the bottom by icebergs was very insignificant indeed, and could in no degree account for the *boulder-clay*, though it was shown that much of the *boulder-drift* in *some places* could be so accounted for. It was, however, demonstrated that there was a great distinction between the boulders which belonged to the *moraine profonde* and those which were carried off on icebergs as part of the ordinary lateral moraines. The fjords, as already partially advocated in a paper in the Journal of the Royal Geographical Society (vol. xxxix.), he considered due to glacial action, the glaciers having taken possession of these fjords when they were mere valleys, when the coast was higher than now. He further showed that the American explorers are in error when they describe the coast of Greenland as rising to the north of 73°, and subsiding to the south of that parallel. There had been a former rise of the coast, and a fall was now in course of progress through the whole extent. Whether these had previously alternated with other rises and falls is not clearly evidenced by remains, but no doubt exists that a rise preceded the present subsidence. Numerous facts were adduced in support of this assertion. The remainder of Dr. Brown's paper was occupied in

an attempt to apply the doctrines regarding the physical action of Arctic ice-action to account for the Scottish glacial remains, and to deduce therefrom evidence regarding the changes Scotland underwent during, and subsequent to, the glacial period.

Aeronautical Society, June 3.—Mr. James Glaisher, F.R.S., in the chair. The following extract from the minutes of a late meeting at Stafford House, was read by the Secretary. It was remarked how little had been done in this country either to demonstrate the possibility of navigating the air, or to prove its impracticability. Sir William Fairbairn observed that we know but little of the reaction or lifting power of various forms of screw blades in the atmosphere, relative to the force employed, though such experiments might be easily tried, and the data obtained. Mr. Brooke was of opinion that if a successful aërial machine were to be constructed, the most simple and obvious plan would be that of inclined surfaces, impelled forward horizontally. The most successful experiment that he had ever witnessed was upon this principle, the motive power being a wound-up watch spring, which, as long as the power lasted, sustained the machine; and further, that most large birds were capable, during long periods of their flight, of sustaining themselves exactly in this way. It was also remarked that we were practically ignorant of the correct laws of the sustaining power of inclined surfaces of different forms and areas, and this want of knowledge was a perpetual stumbling-block to those who were willing to spend time and money in experiments. From the fact that as the weight and size of birds increased, so did the relative wing area decrease, it would appear that the ratio of sustaining surface to weight or resistance was by no means in equal proportions. The Chairman stated that with respect to plane surfaces of various figures exposed to the direct impact of the wind, he had already been trying some experiments with such instruments as were at his disposal, and that by employing two anemometers at the same time, so as to be sure of comparative results, he found that the indication of force increased with the size of the surface; also in the two instruments, equal surfaces shaped into different contours, gave different results. These interesting experiments, so directly bearing upon the question of aërial propulsion and resistance, were still occupying his attention, but at present he could say nothing from actual experiment of the resistance of inclined surfaces of various forms. It was then proposed that an experimental fund should be raised by subscription, and that a suitable and well-finished anemometer should be constructed, having the means of instantly setting various plane surfaces at any desired angle, and capable of registering both horizontal and vertical force simultaneously for all degrees of inclination. The results to be published for the benefit of the Society. Upon this proposition being put to the meeting it was carried unanimously.

Ethnological Society, June 21.—Prof. Huxley, F.R.S., president, in the chair. Colonel Lane Fox made some remarks on the Dorchester dykes and Sinodun Hill, to which attention has recently been directed, and showed that the works are British, and not Roman. He stated that the demolition of these works had been arrested for the present.—Mr. David Forbes, F.R.S., read a paper on the Aymara Indians of Bolivia and Peru. He described them as a small, massive, thick-set race, with large heads and short limbs. The trunk is disproportionately large, and the capacity of the thorax is enormous, being adapted to meet the requirements of respiration in a rarefied atmosphere, as the Aymara lives at an elevation of from 8,000 to 16,000 feet above the sea-level. The proportions of the lower limbs are curious, the thigh being shorter than the leg; the heel is inconspicuous. In colour, the Aymara varies from copper-red to yellowish brown and blackish brown, according to the altitude at which he lives. Many of the customs of the Aymaras depend on their peculiar conditions of life. In consequence of the low boiling-point of water at such great altitudes, beans are rarely used, and the food consists chiefly of potatoes peculiarly prepared. Clay is added to the food, not for any nutritious matter in it, but apparently only to increase the bulk of the meal. In religion, the Aymaras are nominally Christians. They appear to have no system of writing. The discussion on this communication was supported by the President, Mr. E. G. Squier, Mr. Cull, Mr. Dendy, Mr. Bollaert, Mr. Harrison, and Mr. C. Markham. At the same meeting Dr. A. Campbell exhibited tracings of certain rock-inscriptions from British Guiana, and the Hon. E. G. Squier displayed a large collection of drawings, photographs, &c., from Peru.

Anthropological Society of London, June 14.—John Beddoe, M.D., president, in the chair.—Logan D. H. Russell, M.D., of Wilmington, Delaware, was elected a local secretary.—A paper, by Dr. Henry Hudson, was read "On the Irish Celt," in which the author depicted the mental and moral characteristics of that type, and drew conclusions as to the kind of government most suitable to such a people.—Mr. G. H. Kinahan contributed a paper "On the Race Elements of the Irish People." That paper entered largely into the pedigree of the chief families of Connaught and Munster, and treated of the effects of the Cromwellian and other confiscations.—The President (Dr. Beddoe) then read a paper "On the Kelts of Ireland." The principal points proved or indicated in it were the following:—That the Kelts known to the Greek and Latin authors, though they were a light-haired race as compared with the Italians, were darker than the Teutonic tribes, and that their physical type differed in other respects. That the Irish are, generally speaking, a dark-haired but light-eyed race, and that wherever there is much light hair it may be accounted for by a Danish or English cross. That the dark hair of the Irish may be, partly at least, attributed to the Gaelic Kelts. That there is less resemblance between the Irish, taken as a whole, and the Basques, who are generally considered to be the purest Iberians extant, than between the South Welsh and the Basques. That any Basque or Iberian element in Ireland is probably small, and can have only partially contributed to the prevalence of dark hair among the Western Irish. That Ugrian or Ligurian elements may also be present there. The paper was illustrated by minute details respecting the physical types in various parts of modern Ireland, including extensive observations on the colour of the eyes and hair; and the author exhibited a number of photographic and other portraits of Basques and of Bretons, Welshmen, Walloons, and other supposed descendants of the Keltic race.

Meteorological Society, June 15.—Ordinary meeting, Charles V. Walker, F.R.S., president, in the chair. Messrs. W. C. Ellis and Francis Nunes were elected Fellows, and Padre Prof. F. Denza was elected an Honorary Fellow of the society.—The following communications were made: "On the path of the large fireball of November 6th, 1869," by Prof. A. S. Herschel; "On the temperature of the air in Natal, South Africa," by R. J. Mann, M.D., F.R.A.S., &c.; "On the atmospheric pressure with relation to wind and rain," by R. Strachan, F.M.S., and "On the November meteors of 1869, as seen from the Mauritius," by Charles Meldrum, F.R.A.S.—The anniversary meeting was then held, and the report of the council on the present state of meteorological science both at home and abroad, also their report on the present state of the society, which now numbers 343 Ordinary, Life, and Honorary Fellows, and the treasurer's report were then read and adopted. The following is the result of the ballot for the officers and council for the ensuing year.—President, Charles T. Walker, F.R.S., F.R.A.S.; Vice-Presidents, Nathaniel Beardmore, C.E., F.R.S.; C. O. F. Cator; Robert J. Mann, M.D., F.R.A.S.; John W. Tripe, M.D. Treasurer, Henry Perigal, F.R.A.S. Trustees, Sir Antonio Brady, F.G.S., and S. W. Silver. Secretaries, Charles Brooke, F.R.S., F.R.C.S., and James Glaisher, F.R.S., F.R.A.S. Foreign secretaries, Lieut.-Colonel Alex. Strange, F.R.S., F.R.A.S. Council, Arthur Brewin, F.R.A.S., George Dives, F. W. Doggett, Henry S. Eaton, F. Gaster, Charles M. Gibson, Rev. Joseph B. Reade, M.A., F.R.S., W. Wilson Saunders, F.R.S., F.L.S., Thomas Sopwith, F.R.S., George J. Symons, S. C. Whitbread, F.R.S., F.R.A.S., E. O. W. Whitehouse, F.S.A., &c.

Statistical Society, June 23.—William Newmarch, F.R.S., president, in the chair. The following is the list of president, council, and officers, elected to serve for the ensuing year:—President, William Newmarch, F.R.S. Council, Major-General Balfour, C.B., Dr. Thomas Graham Balfour, F.R.S., R. Dudley Baxter, Samuel Brown, Hyde Clarke, D.C.L., L. H. Courtney, Sir C. Wentworth Dilke, Bart., M.P., Dr. W. Farr, F.R.S., W. Fowler, M.P., F. Galton, F.R.S., Right Hon. W. E. Gladstone, M.P., J. Glover, W. A. Guy, M.B., F.R.S., J. T. Hammick, F. Hendriks, J. Heywood, F.R.S., W. Barwick Hodge, F. Jourdan, Prof. Leone Levi, Sir Massey Lopes, Bart., M.P., W. G. Lumley, Q.C., LL.M., J. MacClelland, F. Purdy, Bernhard Samuelson, M.P., Col. W. H. Sykes, M.P., F.R.S., Ernest Seyd, W. Tayler, W. Pollard-Urquhart, M.P., Prof. Jacob Waley, J. Walter, M.P. Treasurer, J. T.

Hammick. Honorary Secretaries, W. Golden Lumley, Q.C., LL.M., F. Purdy, Prof. Jacob Waley.

BRISTOL

The Observing Astronomical Society.—Report of observations made by the members during the period from May 7 to July 6, 1870, inclusive. *Solar Phenomena*—Mr. John Birmingham, of Tuam, writes: "A remarkable obscuration of the sun was observed here on May 22. It lasted from sunrise to sunset, with a short interval in the afternoon of returning brightness. The sun was of a beautiful pink colour, though there was no fog whatever, and its light was so reduced as to permit a long observation of it through the telescope without the aid of a dark glass. I am informed that the same phenomenon was noticed in the South of England on the next day (the 23rd), and on that day also, but late in the afternoon, it was observed at Rohrbach (Moselle), and described by M. Hamant in a letter to the Scientific Association; so that the cause of the obscuration, whatever it was, seems to have been moving eastward and southward." Mr. T. W. Backhouse, of Sunderland, reports that in May "there was a remarkable case of a solar spot making a revolution round another. It occurred with respect to the two largest spots of a group that was half way across the northern zone on May 9. The smaller spot was south of the larger on the 7th at 3^h, but preceded it on the 12th at 21^h, the line joining the two spots having rotated through an angle of 80° or 90° in $5\frac{3}{4}$ days. This movement continued to the 15th, but this would be partly apparent owing to the group approaching the limb. By that time the larger spot was reduced to the size of the other. I cannot say whether the motion was a curve or a straight line, though it was probably the former; nor can I say which of the spots moved or whether both did. They were about 22,000 miles apart on the 9th, at 3^h; but on the 13th, at 20^h, they were 32,000 miles apart. One spot must therefore have moved, relatively to the other, about 34,000 miles in $4\frac{3}{4}$ days, or at the rate of 300 miles per hour." Mr. T. G. E. Elger, of Bedford, says: "The sun spots observed during June were, with the exception of one group, small and devoid of interest when compared with those seen in April and May. The largest spots were confined to the sun's northern hemisphere. Between the 8th and 15th the spots were all small; on the latter date there were only two groups on the disc, and these were insignificant. On the 19th a very remarkable spot was observed, it formed the preceding member of a large scattered group 2′52″ in length; its penumbra measured about 1′ 10″ in greatest diameter. At 10 A.M. an isolated mass of light, intensely bright, was remarked on the nucleus. This, at 2 P.M., formed a 'bridge' connecting adjacent sides of the umbra. The nucleus of this spot was very irregular in colour. At $5^h\ 15^m$ on the 19th the central portion was noted as brown and the border as black, and subsequently the variety of tint was still more marked. At 7 A.M. on the 21st, when the penumbra showed evident signs of cyclonic action, not more than half the area of the nucleus was black, the remainder was made up of patches of various shades of brown. The group disappeared at the limb on the 27th." The Rev. S. J. Johnson, of Crediton, observed numerous spots on the sun on May 13. There were then four groups with penumbræ close together. Mr. H. Michell Whitley, of Penarth, says: "June 21—I noted on the sun's disc one very large, round, and well-defined spot; on one side, however, the penumbra was invaded by two tongues of faculæ for a short distance, and in the centre of the umbra was a bright-patch."

The Planet Saturn.—Mr. H. Michell Whitley repeatedly observed this object with his $6\frac{1}{2}$in. reflector. He says—"June 21. Air very unsteady, but after midnight better. The Ball—duller yellow than rings, equatorial zone yellow, north of this a pale red belt, and another farther north again, towards Pole much fainter and about midway. Pole of planet bluish grey, edges of ball slightly shaded; no other spots or markings. Ring A—Inferior in brightness to B; colour, pale yellow; no subdivisions or markings on it. Ball's division—Traced all round; widest and darkest, if at all, in Wansa; sharply defined. In colour it was not so black as the sky, but deeper than the crape ring across the ball; colour, dusky. Ring B—This ring was very bright for a short distance from its outer edge, which was verp sharply defined; colour gradually deepens and light fades towards inner edge; outer edge lemon yellow, duller and deeper inwards, strongly suspected to be streaky, but no actual subdivision seen. No line of light on inner edge of ring, which was not sharply defined. Ring C, or Crape Ring—Very delicate colour, dusky purple. I could with care, as a very fine object, trace the edge of the globe through it up to ring B, equally distinct in E and Wansa; no markings of any kind upon it.—June 28, 10^h to $11^h\ 15^m$. Power, 250; definition very fluttering. N. equatorial ruddy belt very distinct; equatorial yellow band the brightest part of the planet. Between the N. equatorial ruddy belt and N. Pole lay one or more very faint ruddy bands. Pole, pale bluish grey; no other markings. The Crape Ring very dark and distinct across the Ball.—July 2, 10^h. Definition very sharp; power 250. A glimpse observation. The two belts before mentioned very much plainer and darker than on June 21—28, and not of such a ruddy hue."

Lunar Observations.—Mr. John Birmingham, of Tuam, Ireland, reports that on June 6 he saw "A very marked central depression in the white spot of Linné though the terminator was so far away as the boundary between the Mare and the Palus Putridinus. The depression was rather east of the exact centre of the white spot, so that the western exterior slope was longer than the eastern." Mr. H. Michell Whitley has observed with great care many interesting and difficult lunar objects, and the results of his observations have been sent to Mr. W. R. Birt, F.R.A.S.

Winnecke's Comet.—Mr. George J. Walker, of Teignmouth, observed this body on June 5, 6, and 7. He says that "It looked like a tolerably bright nebula;" on the 6th, at $14^h\ 13^m$ the comet looked faint owing to the strong twilight.

Meteors.—Mr. G. J. Walker saw "a splendid meteor" on June 24. It traversed the greater part of the sky, and was much larger and brighter than Venus. It was of a blue colour. Mr. Walker adds, "I think it appeared a little to the right of Altair, and passed over Vega and on to the Pointers in Ursa Major; it had a magnificent train, and I think must have traversed an arc of about 120°. The time of its appearance, as well as I could make out from my watch, was $11^h\ 13^m$ G.M.T., and it may have been seven or eight seconds making its sweep over the heavens. I did not hear any sound with it." Mr. H. M. Whitley observed a brilliant meteor on June 29 at $11^h\ 30^m$. It was of the second magnitude. "Pale yellow; velocity very great."

A New Red Star.—Mr. John Birmingham has "frequently observed a red star in Cygnus, not, I believe, previously noticed; at least, it is not in Schjelterup's catalogue (Ast. Nach., No. 1,591), which gives a list of all the red stars known up to 1866. It is of a deep red, of about the 8th Mag., and is near a blue star of the same size. Its approximate position, compared with 32 Cygni, is about

$$\text{R.A. } 20^h\ 15^m\ 37^s;\ \text{Declin.} + 47°\ 27'\ 28''.$$

Occultation.—Mr. Walker witnessed the occultation of θ Libræ on June 11, and found that the exact time of disappearance was $9^h\ 27^m\ 55{\cdot}6^s$ G.M.T.

EDINBURGH

Botanical Society, May 12.—Sir Walter Elliot, president, in the chair.—The following communications were read:—Botanical Notes of a Journey through Spain and Portugal, by Mr. T. C. Archer; Botanical Notes on the Garden of Montserrat, Portugal, by Mr. T. C. Archer; Botanical Notes taken on the Rock of Gibraltar, by Mr. T. C. Archer; Report on the open-air Vegetation at the Royal Botanic Garden, by Mr. M'Nab.

June 9.—Sir Walter Elliot, president, in the chair. The following communications were read:—Notes on the Ipecacuanha Plant. By Dr. Gunning, Rio Janeiro. Dr. Gunning states that the Ipecacuanha plant is exceedingly scarce in the province of Rio Janeiro from having been pulled up, and no attention paid to its cultivation. It is exported from Sao Paulo, the province south of Rio, but chiefly from Matto Grosso, a thousand miles up the River Plate. At present Dr. Gunning is rearing a number of cuttings for transmission to India, where it is proposed to cultivate it extensively.—New and rare Mosses from Ben Lawers. By Dr. J. Stirton, Glasgow. In this paper the author reviewed the progress of discovery of mosses on Ben Lawers within the ten years, indicated in general terms the habitats of the rarer species, as well as their tendencies towards increased luxuriance, or gradual decay and extinction, and noticed the affinities between the Cryptogamic Flora of the mountain (Ben Lawers) and that of Scandinavia, more especially of the Dovrefield.—Notice of Grimmias, collected on Arthur Seat, near Edinburgh, by Mr. William Bell and Mr. Sadler. The authors described twelve species and several varieties of the genus Grimmia, as growing on Arthur Seat; noticed their dis-

tribution over the hill, and the kind of rocks on which they occurred.—Notes on some British Mosses. By Mr. Wm. Wilson. Mr. Wilson referred to the British species of *Andræa*, which he had revised for the second edition of his "Bryologia Britannica," and especially to *Didymodon jenneri*, a moss recently described and figured in the Society's Transactions. The latter he believed to be in no way specifically different from *Cynodontium polycarpon*.—On the Ferns found in the Valley of the Derwent. By Mr. T. W. Mawson. Mr. Mawson enumerated twenty-eight species and varieties of ferns as indigenous to the Valley of Derwentwater, including *Asplenium germanicum*, *A. septentrionale*, *Hymenophyllum wilsoni*, *Osmunda regalis*, *Ophioglossum vulgatum*, *Allosorus crispus*, &c.

PARIS

Academy of Sciences, July 18.—M. Bertrand communicated a paper by M. L. Painvin on the determination of the elements of the angle of inflexion of a developable surface defined by its tangential equations.—Several papers on physical subjects were presented, namely—an extract of a letter from M. De la Reve to M. Dumas on the magnetic rotatory powers of liquids; further researches upon electro-capillary action, and on the formation of crystallised oxychloride of copper and other analogous compounds by M. Becquerel; a memoir on the variations of temperature produced by the mixing of two liquids by M. H. Sainte-Claire Deville, in reply to the last communication by M. Jamin, and a reply by the same author to the criticisms of M. Jamin upon a memoir published in 1860; thermical researches upon the metallic character of hydrogen associated with palladium, and on a voltaic couple, in which hydrogen is the active metal by M. P. A. Favre; and a note by M. F. Lucas, communicated by M. E. Becquerel, on the possibility of obtaining fire signals visible at a great disrance, for which purpose the author proposes to employ an electric spark generated by an apparatus described by him.—M. de Saint-Venant presented a memoir on the elementary demonstration of the formula of propagation of a wave or intumescence in a prismatic canal, with remarks on the propagation of sound and light, on ressaults, and on the distinction of rivers and torrents.—A note was read by M. Sonrel on the photographic investigation of the sun at the Imperial Observatory of Paris.—MM. Becquerel and E. Becquerel presented a note on the observations of temperature made beneath the soil at the Garden of Plants from 1864-1870, by means of thermo-electric cables, with tables of results.—The following chemical papers were read:—Investigations upon the action of the chlorides of platinum, palladium, and gold upon the phosphines and arsines, by MM. A. Cahours and H. Gall; a note on the decomposition of oxalic acid by M. P. Carles, communicated by M. Bussy; and a note by M. J. Personne on the conversion of chloral into aldehyde, also presented by M. Bussy.—M. Combes presented a note by M. Flajolot on some crystallised compounds of the oxides of lead and antimony, and of oxide of lead with antimonic acid from the province of Constantine, in Algeria.—A report was read from M. Pasteur on the results of the rearing of silkworms from eggs prepared by processes of selection at Villa Vicentina.—M. C. Robin communicated a note by M. A. Sanson on the influence of the rapid development of the bones upon their density; and M. P. Balestra presented an account of his researches and experiments upon the nature and origin of marsh miasmata, from which he is inclined to believe that the miasmata of marshy places are due to the spores of algæ floating in the air.

VIENNA

Imperial Academy of Sciences, June 17.—Dr. K. Exner communicated a memoir on the sensation of light.—M. J. Schubert communicated drawings and descriptions of a lamp and of an electrical apparatus for producing sound.—M. Tschermak presented a report on the recent fall of a meteorite near Murzuk, in Fezzan.—M. K. Puschl presented a memoir on the amount of heat and the temperature of bodies.—A memoir on reflex action of the nasal mucous membrane upon respiration and the circulation of the blood, by Dr. Kratschmer, was communicated by Prof. E. Hering.—Prof. A. Winckler presented a memoir on the relations between the perfect Abelian integrals of different kinds.—M. von Littrow remarked upon the elements of Winnecke's comet, as calculated by Dr. von Oppolzer.—Prof. Hlasiwetz communicated the results of a long series of experiments made by Dr. Weselsky on the formation of the chinones; and Prof. A. Bauer noticed a compound of platinum and lead having the formula Pt + Pb.

June 23.—The following memoirs were communicated by the Secretary:—On the path of Hind's comet (1847, I.), by Dr. K. Hornstein; on similar conic sections, by M. E. Weyr; and two theories of the movement of free resting masses, by Dr. Recht.—Dr. L. J. Fitzinger communicated the third part of his critical revision of the family of the bats, including the genera *Nyctinomus*, *Thyroptera*, *Exochurus*, *Cnephaiophilus*, and *Vesperus*.—A memoir by Prof. G. Hinrichs (of Iowa), on the statistics of crystalline symmetry, was read, as also a note on the annual course of the temperature at Klagenfurt, Trieste, and Arvavaralja, by Dr. K. Jelinek.

BERLIN

Royal Prussian Academy of Sciences, May 5.—Professor Ehrenberg read a communication on the increasing knowledge of invisible life in the rock-forming *Bacillariæ* of California.

May 12.—Professor Poggendorff read a memoir upon some new and remarkable properties of the diametrical conductors of the electrical machine and on a double machine founded upon these. This paper, which is of considerable length, is illustrated with a figure of the new double machine.—Professor W. Peters read a description, illustrated with figures, of *Platemys tuberosa*, a new species of tortoise from British Guiana.

May 19.—Professor Rammelsbeng read a paper on the composition of the meteorites of Shalka and Hainholz.

May 23.—Professor Ewald read a paper on some questions relating to the geology of the Andes.

GÖTTINGEN

Royal Society of Science, April 6.—A paper by M. W. Krauss on the anterior epithelium of the cornea was read.

April 27.—M. A. Clebsch communicated a paper by Prof. C. Schweigger on the size of the ophthalmoscopic picture, and M. L. Meyer read a note on the occurrence of granular cells in the nervous centres.

May 7.—Prof. Sartorius von Waltershausen read a memoir on the isomorphism of the sulphates of lead, baryta, strontian, lime, potash, soda, and ammonia.—Dr. M. A. Stern presented a simple proof of the law of quadratic reciprocity, and some propositions connected therewith.—M. A. Clebsch read a paper on certain problems of the theory of algebraic surfaces.—M. W. Klinkerfues presented a note of some investigations on the movement of the earth and sun in the æther.—Prof. Enneper read a paper on a problem of mathematical geometry, and Prof. Kohlrausch a notice of the influence of temperature on the coëfficients of elasticity of certain metals.

BOSTON

Natural History Society, *Section of Entomology*, March 23.—Mr. S. H. Scudder in the chair. "Synopsis *Pseudoscorpionidum synonymica*," by Dr. H. Hagen.—Dr. Hagen stated that Dr. A. S. Packard, jun., had recently discovered in Brunswick, Maine, and in Salem, a species of *Amphientomum*, a genus of Neuroptera, whose body is covered with scales, and heretofore known only from Ceylon.—The following paper was read:—"On the Synonymy of *Thecla Calanus*," by Samuel H. Scudder.

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ERRATUM.—Page 235, second column, line 24, for "Caprera" read "Capri."